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Stops and Stares: Street Stops, Surveillance, and Race in the New Policing

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STOPS AND STARES: STREET STOPS, SURVEILLANCE, AND RACE IN THE NEW POLICING*

Jeffrey Fagan,** Anthony A. Braga,*** Rod K. Brunson,**** and April
Pattavina*****

ABSTRACT

The use of proactive tactics to disrupt criminal activities, such as Terry street stops and concentrated misdemeanor arrests, are essential to the “new policing.” This model applies complex metrics, strong management, and aggressive enforcement and surveillance to focus policing on high crime risk persons and places. The tactics endemic to the “new policing” gave rise in the 1990s to popular, legal, political, and social science concerns about disparate treatment of minority groups in their everyday encounters with law enforcement. Empirical evidence showed that minorities were indeed stopped and arrested more frequently than similarly situated Whites, even when controlling for local social and crime conditions. In this Article, we examine racial disparities under a unique configuration of the street stop prong of the “new policing”—the inclusion of non-contact

* This research was conducted at the joint request of the Boston Police Department and the ACLU of Massachusetts. The Boston Police Department generously provided the data on its encounters with citizens as well as data on crime and arrests and police officers in the City of Boston. We thank Police Commissioner William Evans, former Commissioner Edward Davis, former Chief of Staff Sharon Hanson, and the staff of the Boston Regional Intelligence Center. At the ACLU of Massachusetts, John Reinstein helped create the collaboration that made possible this research. Carol Rose and Matthew Segal of the ACLU also contributed to the project. All opinions are solely ours and do not reflect the views of the Boston Police Department or the ACLU.

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observations (or surveillances) in the field interrogation and observation activity of Boston Police Department officers. We show that Boston Police officers focus significant portions of their field investigation activity in two areas: suspected and actual gang members, and the city's high crime areas. Minority neighborhoods experience higher levels of field interrogation and surveillance activity, controlling for crime and other social factors. Relative to White suspects, Black suspects are more likely to be observed, interrogated, and frisked or searched controlling for gang membership and prior arrest history. Moreover, relative to their Black counterparts, White police officers conduct high numbers of field investigations and are more likely to frisk or search subjects of all races. We distinguish between preference-based and statistical discrimination by comparing stops by officer-suspect racial pairs. If officer activity is independent of officer race, we would infer that disproportionate stops of minorities reflect statistical discrimination. We show instead that officers seem more likely to investigate and frisk or search a minority suspect if officer and suspect race differ. We locate these results in the broader tensions of racial profiling that pose recurring social and constitutional concerns in the "new policing."

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INTRODUCTION

A. The New Policing

In an essay published in 2000 in the *Fordham Urban Law Journal*, Professor Philip Heymann credited the “new policing” for the sharp crime declines of the preceding decade.¹ Three essential features characterized the “new policing.” First, police innovators developed real-time policing metrics both for internal personnel management and to inform how and where police are deployed across their respective cities.² They noted the importance of the strategic concentration of police resources in the city’s highest crime areas based on new methods of crime mapping and analysis.³ Second, these metrics were used to hold local police commanders accountable for

1. See Phillip B. Heymann, *The New Policing*, 28 FORDHAM URB. L.J. 407, 411, 413–14 (2000). A few years before, Professor Debra Livingston had summarized this vector of tactics that comprised the “new policing” in a 1997 essay, but stopped short of crediting the police for producing the crime decline across cities. Debra Livingston, *Police Discretion and the Quality of Life in Public Places: Courts, Communities, and the New Policing*, 97 COLUM. L. REV. 551, 672 (1997).

2. Heymann, *supra* note 1, at 429–32.

3. David Weisburd et al., *Reforming to Preserve: Comp Stat and Strategic Problem Solving in American Policing*, 2 CRIMINOLOGY & PUB. POL’Y 421, 440–41 (2003) [hereinafter Weisburd et al., *Reforming to Preserve*]. For early experiments on metrics-driven proactive policing, see generally Lawrence W. Sherman & David Weisburd, *General Deterrent Effects of Police Patrol in Crime “Hot Spots”: A Randomized, Controlled Trial*, 12 JUST. Q. 625 (1995); Edward McGarrell et al., *Reducing Firearms Violence Through Directed Police Patrol*, 1 CRIMINOLOGY & PUB. POL’Y 119–148 (2001–02); David Weisburd et al., *The Possible “Backfire” Effects of Hot Spots Policing: An Experimental Assessment of Impacts on Legitimacy, Fear and Collective Efficacy*, 7 J. EXPERIMENTAL CRIMINOLOGY 297 (2011).

crime trends in their precincts.⁴ Failure to lower crime rates resulted in a form of public shaming in meetings of high-level police executives, and possibly demotion and re-assignment.⁵ Accountability and heightened management control were essential tools to incentivize commanders to closely watch and react to local crime conditions.

The third new policing tool was the use of proactive tactics to disrupt criminal activities. Aggressive street stops were one of the tactics highlighted by both Livingston and Heymann.⁶ They and others⁷ discussed the new police focus on order maintenance and the aggressive enforcement of quality-of-life crimes,⁸ on the enforcement of minor crimes as a way to suppress more serious crimes, on stop and frisk tactics to disrupt crimes and especially to seize weapons, and on new developments in community policing that brought police into closer collaborative relationships with citizens.⁹

Decades earlier, the U.S. Supreme Court had expanded the legal boundaries under which police could conduct field interrogations, or street stops, in a 1968 decision, *Terry v. Ohio*.¹⁰ *Terry* permitted temporary stops and detentions based on reasonable suspicion that crime was “afoot,” supplanting the more demanding probable cause standard¹¹ and memorializing police discretion as the gateway to

4. WILLIAM BRATTON & PETER KNOBLER, *THE TURNAROUND: HOW AMERICA'S TOP COP REVERSED THE CRIME EPIDEMIC* (1998).

5. JOHN F. TIMONEY, *BEAT COP TO TOP COP: A TALE OF THREE CITIES* 168–69 (2010).

6. Livingston, *supra* note 1, at 571.

7. *See, e.g.*, GEORGE KELLING & CATHERINE COLES, *FIXING BROKEN WINDOWS* (1996); Anthony A. Braga et al., *The Effects of Hot Spots Policing on Crime*, 31 JUST. Q. 633, 658–60 (2014); David Thacher, *Order Maintenance Reconsidered: Moving Beyond Strong Causal Reasoning*, 94 J. CRIM. L. & CRIMINOLOGY 381, 392–94 (2004).

8. *See supra* note 7. The policy also focused intensively on misdemeanor drug crimes, especially marijuana possession, as part of the “Broken Windows” strategy. Part of the logic of such enforcement was also to find more serious criminal offenders among those committing such minor crimes. *See generally* JACK MAPLE & CHRIS MITCHELL, *THE CRIME FIGHTER: HOW YOU CAN MAKE YOUR CRIME FREE* (2000). Maple and Mitchell claimed that marijuana arrests would prevent more serious crimes since marijuana smokers were criminals “on their day off.” *Id.* at 155.

9. *See supra* note 7.

10. 392 U.S. 1, 21–22 (1968) (holding that “[t]he reasonableness of any particular search and seizure must be assessed in light of the particular circumstances . . . [against the standard of whether] a man of reasonable caution is warranted in believing that the action taken was appropriate.”); *see also infra* Part II and accompanying notes.

11. *Maryland v. Pringle*, 540 U.S. 366, 366–67 (2003) (reaffirming the probable cause standard under the Fourth Amendment to justify a “search and seizure”).

street stops.¹² Subsequent decisions further expanded the tolerances around the concept of “reasonable suspicion.”¹³ The “new policing” embraced the use of street stops as a critical tool to disrupt criminal activity, despite the absence of any evidence of the comparative advantage of street stops over other policing tactics.¹⁴

Proactivity was the animating theory of the “new policing,” whether in the context of data-driven management metrics such as CompStat,¹⁵ a computerized crime accounting system, the aggressive use of arrests for minor crimes, or the conduct of street stops at the first signs of suspicious behavior.¹⁶ In the current era, the term was first used without fanfare by Professors Jerome Skolnick and David Bayley in their description of policing innovations in the 1980s.¹⁷ New York City police first used proactive policies to disrupt open-air drug markets starting in the early 1990s.¹⁸

Over time, proactivity became a broad umbrella for a wide range of police tactics. One study defines “proactive policing” as the vigorous enforcement of law against minor (misdemeanor) offenses.¹⁹ Other studies mention the use of stop and frisk, or investigative stops, as central to a proactive policing policy.²⁰ Still others portray a curious admixture of drug enforcement and community policing as

12. *See generally Terry*, 392 U.S. 1.

13. *See infra* Part II and accompanying notes.

14. A decade later, the National Research Council policing panel conspicuously avoided the question of what police should do once they got to the targeted places. However, a recent systematic review and meta-analysis of the effects of hot spots policing on crime suggests that community problem-solving approaches generated stronger crime reduction impacts relative to increased enforcement efforts. *See Braga et al., supra* note 7, at 633.

15. Weisburd et al., *Reforming to Preserve*, *supra* note 3, at 424. Many police departments adopted elements of the “new policing” without incorporating the metrics-driven management algorithms for targeting and assessment of police actions. In fact, the authors critique management metrics as retarding organization reform and reinforcing the paramilitary model of police innovation. *Id.*

16. *Id.* at 427.

17. *See generally* JEROME SKOLNICK & DAVID BAYLEY, *THE NEW BLUE LINE: POLICE INNOVATION IN SIX AMERICAN CITIES* (1986) (discussing a shift in police tactics from being reactive to crime complaints toward acting in response to chronic criminal problems in specific places).

18. Lynn Zimmer, *Proactive Policing Against Street-Level Drug Trafficking*, 9 AM. J. POL’Y 43, 52–55 (1990).

19. Charis Kubrin et al., *Proactive Policing and Robbery Rates Across U.S. Cities*, 48 CRIMINOLOGY 57, 57 (2010).

20. JEFFREY FAGAN ET AL., *Street Stops and Broken Windows Revisited: The Logic and Demography of Proactive Policing in a Safe and Changing City*, in RACE, ETHNICITY, AND POLICING 309 (Stephen Rice & Michael White eds., 2010).

“proactive.”²¹ Accordingly, there is no consensus on what constitutes “proactive policing” other than its emphasis on anticipation of criminal activity and directing action to those places or persons, and its commitment to systematic criminal enforcement of minor crimes.²² Tactics such as investigative stops (stop and frisk, or *Terry* stops), order maintenance and aggressive responses to quality of life enforcement, the same tactics cited by Livingston, are basic to many conceptions of proactive policing, and are the focus of this Article.²³

B. Policing Crime, Policing Race

The metrics of the “new policing” pointed to the neighborhoods with the highest crime rates as the targets of police activity. These usually were the places with concentrated poverty and often were minority neighborhoods.²⁴ At first glance, this seems a rational and proportional response, consistent with most benchmarking strategies to assess fairness or bias.²⁵ Yet, regardless of the distribution and allocation function to assign police to neighborhoods—a linear allocation of police to neighborhoods based on differences in their crime rates, for example—disproportionate allocations raise both

21. Jon Gould & Stephen Mastrofski, *Suspect Searches: Assessing Police Behavior Under the U.S. Constitution*, 3 CRIMINOLOGY & PUB. POL’Y 315, 318 (2004).

22. The original “broken windows” essay, whose ideas informed much of the next decade of proactive policing, suggested that arrest was a last resort if other efforts failed to ameliorate the disorderly conditions that invited crime. See George L. Kelling & James Q. Wilson, *Broken Windows: The Police and Neighborhood Safety*, ATLANTIC (March 1982), <http://www.theatlantic.com/magazine/archive/1982/03/broken-windows/304465/> [https://perma.cc/LW3A-6JHA]. By 2000, Kelling had embraced the notion of using arrest authority systematically and aggressively to stop minor crime from growing into more serious crime patterns and problems. See KELLING & COLES, *supra* note 7, at 108–156.

23. Livingston, *supra* note 1, at 554–58.

24. Jeffrey Fagan & Garth Davies, *Street Stops and Broken Windows: Terry, Race, and Disorder in New York City*, 28 FORDHAM URB. L.J. 457, 462 (2000); see Lauren J. Krivo et al., *Segregation, Racial Structure, and Neighborhood Violent Crime*, 114 AM. J. SOC. 1765, 1765–1802 (2009); see also ROBERT SAMPSON & JEFFREY MORENOFF, DURABLE INEQUALITY: SPATIAL DYNAMICS, SOCIAL PROCESSES AND THE PERSISTENCE OF POVERTY IN CHICAGO NEIGHBORHOODS 176 (Samuel Bowles et al. eds., 2006).

25. See Greg Ridgeway & John MacDonald, *Methods for Assessing Racially Biased Policing*, in RACE, ETHNICITY, AND POLICING: NEW AND ESSENTIAL READINGS 180 (Stephen Rice & Michael White eds., 2010); Jeffrey Fagan, *Law, Social Science, and Racial Profiling*, 4 JUST. RES. AND POL’Y 104 (2002); Ian Ayres, *Outcome Tests of Racial Disparities in Police Practices*, 4 J. JUST. RES. & STAT. ASS’N 131 (2002); see also Lawrence Rosenthal, *The Crime Drop and the Fourth Amendment: Toward an Empirical Jurisprudence of Search and Seizure*, 29 N.Y.U. REV. L. & SOC. CHANGE 641 (2004).

fairness and efficiency questions.²⁶ In such instances, minority citizens are exposed to “more” policing than their crime conditions would dictate, and persons in other neighborhoods placed at risk due to under- or de-policing of their neighborhoods.²⁷

Assuming limits on the effectiveness of police in an area—after all, there is only so much crime to go around—then over-policing risks adverse consequences from unnecessary and unproductive police contacts.²⁸ And since these stops are neither pleasant nor without consequences,²⁹ allocations framed this way raise constitutional questions of disparate treatment.³⁰ The persistence of these errors in the context of the extensive use of noxious tactics suggests that these practices and disparities took on the characteristics of a government program rather than the exercise of individual officers’ judgment and discretion.³¹

Court rulings often skirt the question of whether bias is the dynamic that produces disparities, preferring instead to examine discriminatory intent. The Fourteenth Amendment’s Equal Protection Clause forbids state actors from denying the equal protection of the law.³² Intentional discrimination by race is the standard, not simply whether a policy or practice has a disproportionate racial impact.³³ Whether that intent is a matter of bias or preferences is not central to a legal determination. Intent, instead, is the predicate to determine discrimination. Courts have developed standards to establish discriminatory intent that would

26. *See supra* note 25.

27. *See id.*

28. *See id.*

29. *See* Tom R. Tyler, Jeffrey Fagan & Amanda B. Geller, *Street Stops and Police Legitimacy: Teachable Moments in Young Urban Men’s Legal Socialization*, 11 J. EMPIRICAL LEGAL STUD. 751, 758 (2014); Amanda Geller, Jeffrey Fagan & Tom R. Tyler, *Aggressive Policing and the Mental Health of Young Urban Men*, 104 AM. J. PUB. HEALTH 2121, 2121 (2014).

30. *See* *Floyd v. City of New York*, 959 F. Supp. 2d 540, 570 (S.D.N.Y. 2013); *see also* Jeffrey Fagan, Greg Conyers & Ian Ayres, *No Runs, Few Hits and Many Errors: Street Stops, Bias and Proactive Policing* (2014) (unpublished conference draft) (on file with author).

31. TOM R. TYLER & YUEN J. HUO, *TRUST IN THE LAW: ENCOURAGING PUBLIC COOPERATION WITH THE POLICE AND COURTS* (2002); RONALD WEITZER & STEVEN TUCH, *RACE AND POLICING IN AMERICA: CONFLICT AND REFORM* (2006); David Harris, *The Stories, the Statistics, and the Law: Why “Driving While Black” Matters*, 84 MINN. L. REV. 265, 267–68 (1999); Tracey L. Meares, *Programming Errors: Understanding the Constitutionality of Stop-and-Frisk as a Program, Not an Incident*, 82 U. CHI. L. REV. 159, 164–65 (2015).

32. U.S. CONST. amend. XIV, § 1.

33. *See* *Washington v. Davis*, 426 U.S. 229, 239–40 (1976).

satisfy an equal protection claim, such as intentionally classifying persons by race for differential treatment.³⁴ The standard most applicable for contemporary policing is an “as applied” determination: that a seemingly neutral policy is applied in an intentionally discriminatory manner.³⁵ Courts have argued that an equal protection claim is satisfied by evidence of a discriminatory “purpose” as a “motivating factor” for the practice under scrutiny.³⁶ The question of bias is secondary to the question of the complex task of discerning discriminatory intent. In the case of a widespread program³⁷ of *Terry* or street stops, an intentionally disproportionate application of the stop authority to persons of one race—with perhaps deliberate indifference to these patterns—raises the specter of intent.³⁸

Still, there remain several empirical challenges to detecting bias in the institutional practices of law enforcement agencies and in the actions of individual police officers. Control groups, benchmarks to assess proportionality, endogeneity of crime and policing, varying spatial boundaries, individual biases versus institutional practices, police-citizen interpersonal interactions, characteristics of officers and the composition of both the civilian and police populations, multiple outcomes of stops and processes within stops, and several important contextual factors all contribute to the challenges facing researchers. All these parameters are basic to design decisions in assessing the extent of police discrimination. We address these challenges in the empirical project reported in this Article.

34. See, e.g., *Brown v. Oneonta*, 221 F.3d 329, 333–34 (2d Cir. 2000) (permitting a racial classification for police only when police are pursuing a person meeting a specific suspect description). Normally, all such classifications are evaluated at the highest standard of “strict scrutiny.” See, e.g., *Johnson v. California*, 543 U.S. 499, 505 (2005) (holding that “all racial classification” imposed by government “must be analyzed by a reviewing court under strict scrutiny”); *Loving v. Virginia*, 388 U.S. 1, 11 (1967).

35. See, e.g., *Brown*, 221 F. 3d at 337; see also *Yick Wo v. Hopkins*, 118 U.S. 356, 373–74 (1886).

36. See *Floyd v. City of New York*, 959 F. Supp. 2d 540, 571 (S.D.N.Y. 2013) (quoting *Village of Arlington Heights v. Metropolitan Hous. Dev. Corp.*, 429 U.S. 252, 265–66 (1977) (“Because discriminatory intent is rarely susceptible to direct proof, litigants may make “a sensitive inquiry into such circumstantial and direct evidence of intent as may be available. The impact of the official action—whether it bears more heavily on one race than another—may provide an important starting point.”)).

37. Meares, *supra* note 31, at 164–65.

38. *Id.*

C. Stops and Stares: The Surveillance Prong of the “New Policing”

In this Article, we examine the extent of racial disparities under a unique configuration of the street stop prong of the “new policing”—the use of Field Interrogation, Observation, Frisk and/or Search (BPD form 2487; hereafter, FIO) reports by the Boston Police Department (BPD). As in many other big cities, the Boston FIO strategy included the basic element of investigative stops or field interrogations as a staple of its proactive tactics.

Beyond this core tactic, however, two additional components distinguish the Boston design from most forms of the “new policing.” First, Boston officers conduct non-contact observations of known criminal offenders or others gathering in known crime locations. Officers are required to complete FIO reports for both in-person encounters and non-contact observations. Officers are required to enter the information from non-contact observations or surveillance in the same databases that house data from field interrogations (or investigative stops).³⁹ Department policy requires that observations be more than a stakeout or a hunch. That is, the observation must be specific to a suspected crime, rather than general surveillance of individuals.⁴⁰

Second, the policy explicitly recognizes the role of surveillance and intelligence-gathering in the local strategy.⁴¹ Surveillance of known offenders, their associates, and their gathering places “plays an important part in the department’s intelligence efforts to collect and disseminate data on the activities and whereabouts of known and suspected criminals and their associates in the city.”⁴² In effect, the strategy allows the department to build a network database of the movements and associations of individuals through time and space in

39. *See, e.g.*, Bos. Police Dep’t, Special Order SO 05-023, June 3, 2005, § 1 (“An officer should . . . complete an FIOFS report whenever (a) he/she observes an individual who [sic] the officer knows to have a criminal record.”).

40. *Id.* at § 4 (noting that “[a]n officer shall complete an FIOFS report whenever: (a) he/she observes, detains, or interrogates a person suspected of unlawful design The officer must enter the type of crime suspected [I]t is not sufficient to simply enter ‘suspicious person’ or ‘SP.’”); *see also* Bos. Police Dep’t, *Rules and Procedures, Rule 323*, § 5 (March 9, 2011).

41. Special Order SO-5-023, at § 1 (stating that “[t]he FIOFS report has been prepared so that the department may accumulate up-to-date information concerning known criminals and their associates, the vehicles they use, the places they frequent, and persons suspected of unlawful design”).

42. *Id.* at General Considerations.

the city. The observations also are considered documentary evidence and therefore admissible if relevant in any future proceedings.⁴³

Few police departments have acknowledged the potential for intelligence-gathering that is created from *Terry* stops, much less how the data could be aggregated and analyzed for that purpose. Even fewer acknowledge the Fourth Amendment implications on privacy and anonymity.⁴⁴ While intelligence-gathering is not unusual in policing in response to known crime groups such as street gangs or drug selling organizations,⁴⁵ the transformation of *Terry* stops into an intelligence regime seems new. While surveillance of individuals or gangs is permissible, surveillance without a warrant for places and persons wandering through them raises constitutional concerns.⁴⁶

Pretextual stops made on thin grounds for reasonable suspicion can create opportunities to record time-place-network activities of suspected offenders or other valued targets such as gang members or drug traffickers. Surveillance without contact is another matter.⁴⁷ Recorded observations by officers who have knowledge of the identity and affiliations of that target can in effect double down on the information gathered through in-person contacts.⁴⁸ These non-contact observations can in fact lead to further contacts, assuming

43. Bos. Police Dep't, *Rules and Procedures*, Rule 323, § 2.

44. See CHRISTOPHER SLOBOGIN, *PRIVACY AT RISK: THE NEW GOVERNMENT SURVEILLANCE AND THE FOURTH AMENDMENT* 79 (2007) (discussing the elasticity of the reasonable suspicion prong of Fourth Amendment doctrine to accommodate contemporary surveillance practices by police of gang members and others suspected of potential criminal participation).

45. See William Bloss, *Escalating U.S. Surveillance After 9/11: An Examination of Causes and Effects*, 4 SURVEILLANCE & SOC'Y 208 (2002) (documenting the expansion of police surveillance in response to perceived threats from crime, drug selling and national security concerns); see also Debra Livingston, *Gang Loitering, the Court, and Some Realism about Police Patrol*, 199 SUP. CT. REV. 141, 144 (1999) (expressing concern that granting police authority to focus on public spaces can evolve into policing as an instrument for surveillance and harassment of "disfavored" individuals or groups).

46. See *Katz v. United States*, 389 U.S. 347, 350 (1967) (finding that there is no Fourth Amendment violation when evidence is obtained without "physical entrance into the area"); Orin Kerr, *The Fourth Amendment and New Technologies: Constitutional Myths and the Case for Caution*, 102 MICH. L. REV. 801 (2004). But see *Kyllo v. United States*, 533 U.S. 27, 34–35 (2001) (holding that the use of a thermal imaging device from a public position to monitor the radiation of heat from a person's home was a "search" under the Fourth Amendment, and thus required a warrant).

47. See, e.g., R. Richard Banks, *Beyond Profiling: Race, Policing and the War on Drugs*, 56 STAN. L. REV. 571 (2003); Jeffrey Fagan & Amanda B. Geller, *Following the Script*, 82 U. CHI. L. REV. 51 (2015).

48. See, e.g., Banks, *supra* note 47; Fagan & Geller, *Following the Script*, *supra* note 47.

usable intelligence that can be linked to specific persons or groups.⁴⁹ For example, the U.S. Customs Service, under former NYPD Commissioner Raymond Kelly, used “intelligence developed by another officer” to target persons for later administrative searches.⁵⁰

The observational or intelligence components of the “new policing” have received little attention in either legal or empirical scholarship on policing, or on the constitutional implications of these types of “passive” stops. Certainly, there are Fourth Amendment implications when police use prior suspicionless *observations* as a partial basis for a later interdiction that risks arrest and its aftermath.⁵¹ Chicago’s constitutional troubles in the 1990s with its gang loitering ordinance show the difficult line that police face when determining when to escalate observation of non-criminal conduct such as loitering into reasonable suspicion of a crime that would justify a coercive stop.⁵² If there is no notice to citizens about what conduct might authorize police attention, it may be equally unclear to the police.

There also are distinct Equal Protection implications when individuals are disproportionately targeted by race for observations that can lead to more intrusive or coercive police actions in the short term.⁵³ Racial disparities in this passive component of the new policing would be compounded in subsequent police contacts, given the leveraging of intelligence into further stop activity.⁵⁴ The deeper

49. Because there is no seizure of the subject, *Terry*’s ground rules for street detentions may not be applicable to non-contact observations. Yet observations can be bootstrapped by police officers into information that can serve as the basis for the reasonable suspicion that *Terry* requires. See *Terry v. Ohio*, 392 U.S. 1, 27–28 (1968).

50. Jerome Skolnick & Abigail Caplovits, *Guns, Drugs and Profiling*, 43 ARIZ. L. REV. 413, 433–34 (2001) (citing UNITED STATES GENERAL ACCOUNTING OFFICE, BETTER TARGETING OF AIRLINE PASSENGERS FOR PERSONAL SEARCHES COULD PRODUCE BETTER RESULTS, GAO/GGD-00-38, 87, at 5–6, 16 (2000)).

51. See *Commonwealth v. Cruz*, 945 N.E.2d 899, 908 (Mass. 2011). Cruz was ordered to exit a vehicle when police officers spotted it parked next to a fire hydrant and then smelled marijuana smoke. Possession of small amounts of marijuana had been decriminalized in Massachusetts since 2008. Cruz had fallen under the police gaze in this instance because of his prior encounters with police, and those encounters heightened their suspicion leading to the search of the vehicle for what amounts to a civil infraction. See also David Keenan & Tina M. Thomas, *An Offense-Severity Model for Stop and Frisks*, 123 YALE L.J. 1448, 1458–60 (2014).

52. *Chicago v. Morales*, 521 U.S. 41, 55–57 (1999) (holding that a gang loitering ordinance is vague as to what behaviors would be actionable for police and therefore encourages arbitrary and discriminatory enforcement); see Livingston, *Gang Loitering*, *supra* note 45, at 163 and accompanying text.

53. Jeffrey Fagan, *Law, Social Science, and Racial Profiling*, *supra* note 25, at 106–08.

54. See, e.g., *id.* at 111–12.

(though not wider)⁵⁵ pool of information may well increase the odds of an in-person contact, raising additional questions about disparities and the potential for a constitutional claim.

Surveillance is fundamental to the street stop component of the “new policing.” The sequence of interactions leading to an investigative stop, or a contact stop, often include a period of observation of a suspect before an officer decides if there is sufficient suspicion under the Fourth Amendment to proceed to contact and interaction with the suspect.⁵⁶ In most places, these predicate observations are not recorded if the surveillance does not convert to a contact stop; only those observations that trigger actions are included in databases.⁵⁷ This is what sets the Boston FIO regime apart from other forms of the new policing: its expansion under Fourth Amendment ground rules to include surveillance of the type that normally is reserved for national security concerns or complex criminal organizations.⁵⁸ Here, the everyday movements of persons fall under the police gaze and are memorialized in databases. Whether these observations are contributory to disparate treatment under the Equal Protection clause on their own or in conjunction with direct contacts is the focus of this Article.

D. This Article

Empirical tests for Equal Protection violations in policing have become more common and urgent as political and legal challenges to the “new policing” have grown.⁵⁹ Research to date on vehicle and

55. The pool is deeper in the sense that more information is gathered about the same person or persons over time. But if an area or group is targeted, the information is narrowly focused on one person or one social network or one or more groups of persons moving through a targeted space. While *Illinois v. Wardlow* requires presence in a high crime area as a component of suspicion, passive stops based on presence in a high crime area de-temporize that presence and disconnect it from other indicia of suspicion that are present at the time of the street stop. See *Illinois v. Wardlow*, 528 U.S. 119, 123 (2000).

56. See Fagan & Geller, *Following the Script*, *supra* note 47, at 63.

57. See, e.g., N.Y. Police Dep’t, *The Spot, Frisk, and Question Database* (2003–2015), http://www.nyc.gov/html/nypd/html/analysis_and_planning/stop_question_and_frisk_report.shtml [<https://perma.cc/9A8T-FNS6>]. The databases include only in-person stops or field interrogations. For each stop, the period of observation preceding the stop is also recorded. Nearly all of the observation periods are less than two minutes. Fagan & Geller, *Following the Script*, *supra* note 47, at 63.

58. See Bos. Police Dep’t, *Rules and Procedures*, Rule 323, § 1; see, e.g., Daniel Richman, *The Right Fight*, BOS. REV. (Dec. 1, 2004), <https://bostonreview.net/forum/right-fight> [<https://perma.cc/7MDP-NL7B>].

59. Sonja B. Starr, *Explaining Race Gaps in Policing: Normative and Empirical Challenges* 3 (U Mich. L. & Econ., Res. Paper No. 15-003, 2015).

highway stops raises difficult questions as to the construction and vetting of claims of racial discrimination.⁶⁰ These difficulties have been compounded and perhaps have become more complex with the increased attention to policing inequalities by the U.S. Department of Justice⁶¹ and civil litigation on a variety of statutory and constitutional grounds.⁶² These cases have generated new databases and competing analytic strategies to identify the causal role of race in the observed disparities in policing. In this Article, we examine the role of race in explaining how pre-arrest coercive policing and passive surveillance-based policing are carried out in an urban setting of complex and varied crime problems.

The addition of passive or surveillance-based policing models expands the underlying normative tensions in policing by placing citizens under the police gaze in a setting more closely approximating a panopticonistic vision of policing.⁶³ Police surveillance of organized crime groups and political dissidents has a long history, with court interventions that established procedural and substantive boundaries on these activities.⁶⁴ Incorporating surveillance into a prophylactic *Terry* regime, whether for street gangs or other loosely organized offender networks or for everyday criminal or delinquent activity, merges acute public safety and national security concerns into everyday policing.⁶⁵ This spillover from national security to the “new policing,” raises important questions about how the equal protection doctrine applies. The further dimension of racial disparity that seems

60. *Id.* at 34–36.

61. See Rachel Harmon, *Promoting Civil Rights Through Proactive Policing Reform*, 62 STAN. L. REV. 1, (2009) (discussing the limitations of “retail” litigation under §1983 and other individual cases to produce institutional reform in policing and arguing for the stronger, institutionally-focused mechanisms of 42 U.S.C. § 14141 to produce meaningful institutional changes).

62. See Joanna Schwartz, *Police Indemnification*, 89 N.Y.U. L. REV. 885 (2014) (showing results of litigation under Monell liability claims for police misconduct).

63. See William Simon, *In Defense of the Panopticon* (Colum. Pub. L., Res. Paper No. 14-412, 2014) (arguing for the social welfare and regulatory benefits of neutral broad surveillance including the police as governmental actors); see also Peter K. Manning, *A View of Surveillance*, in *TECHNOCRIME: TECHNOLOGY, CRIME AND SOCIAL CONTROL*, 209–42 (2008). See generally Larry Catá Backer, *Global Panopticism: States, Corporations, and the Governance Effects of Monitoring Regimes*, 15 IND. J. GLOBAL LEGAL STUD. 101, 112 (2008) (tracing the modern decentralized and globalized surveillance state).

64. See Benjamin S. Mishkin, *Filling the Oversight Gap: The Case for Local Intelligence Oversight*, 88 N.Y.U. L. REV. 1414, 1427 n.78 (2013).

65. See *id.*; see also, Richman, *supra* note 58.

inherent in street stop regimes, deepens the stakes in this analysis of the “new policing” by linking race and national security rationales.⁶⁶

The Article unfolds in the following five parts. Part I provides background on the transformation of police and the emergence of proactive policing since *Terry*. Despite the efforts of the *Terry* court to scrub race from its analysis of the original *Terry* stop, the role of race in the jurisprudence of *Terry* stops has become clearer over time, leading to the important cases today that closely connect *Terry*, race and proactive policing. We trace the political and normative conflicts over racial profiling into the courts, and also into the sociological analysis of race and policing. We draw a distinction between discrimination and bias, and show the divide in empirical strategies to test the two explanations of observed racial disparities.

Part II describes in detail the research site, and the integration of surveillance into the regime of proactive policing in Boston. Part III discusses the research strategy to identify the role of race in producing the patterns of policing that we observe in Boston. We weigh alternatives facing empirical researchers in measurement and analysis of these data and explain the methodological choices that we made. We distinguish the role of race in surveillance versus in-person encounters. We discuss the use of officer race as a metric to identify the extent of racial preferences among officers. We also discuss modeling choices for data where policing is spatially disaggregated across parts of the city that vary extensively in their social and crime condition.

Part IV presents the empirical results. We show that contact stops and non-contact observations of Black suspects are more common than for White suspects after controlling for local crime and social conditions in Boston neighborhoods. The patterns are robust to several alternate empirical specifications. The local emphasis on surveillance and interdiction of gang members explains some but not all of the racial disparities in the conduct of FIOs; these disparities are present across most tactical units and locales in the BPD. Diversity in policing matters in these estimates: Black officers appear to be less active in FIO reporting than their White colleagues, both in general patrol activities as well as in the specialized gang enforcement units. They make fewer stops of Black suspects and search Black suspects less often. The results suggest that Black and Hispanic officers seem to act with statistical discrimination whereas White officers seem to

66. See, e.g., Stephen J. Schulhofer, Tom R. Tyler & Aziz Z. Huq, *American Policing at a Crossroads: Unsustainable Policies and the Procedural Justice Alternative*, 101 J. CRIM. L. & CRIMINOLOGY 335, 356 (2013)

act based on preferences for discrimination that are present beyond what statistical discrimination would suggest. We show continuity in racial disparities in police contacts from the general *Terry* regime of street stops to the expanded surveillance activities, indicative of the broader expansion of *Terry* doctrine over the past half century.

We conclude with a discussion of two intersecting implications of this case study: efficiency and fairness. As a preliminary question, we assess the adequacy and probative value of this empirical strategy to detect equal protection violations in the conduct of stops coupled with a more arms-length program of surveillance. The new policing inevitably will produce racially disparate impacts, given crime patterns and the actuarialism that is baked into its strategy and logic. Its expansion to surveillance, which sidesteps *Terry*'s rules, raises new questions about the constitutional regulation of the "new policing." The devolution of *Terry* to a program of both surveillance and administrative stops raises important questions for the regulation of this activity and more broadly for the governance of police with respect both to privacy considerations and for its racial dimensions.⁶⁷

I. TERRY STOPS, RACE, AND THE NEW POLICING

A. Expansion and Deregulation of Investigative Stops

In *Terry v. Ohio*,⁶⁸ the Supreme Court granted police broad authority to conduct investigative stops when they have reasonable and particularized suspicion to believe that crime is imminent, in progress, or has just occurred.⁶⁹ The issues of race that were minor features of the original *Terry* case became explicit concerns over the next several decades in the law, politics, and policy of policing.⁷⁰

67. William J. Stuntz, *The Distribution of Fourth Amendment Privacy*, 67 GEO. WASH. L. REV. 1265, 1272–73 (1999).

68. See *Terry v. Ohio*, 392 U.S. 1 (1968).

69. *Id.* at 30 (holding that officers could temporarily detain and question a suspect when they had reasonable and particularized suspicion to believe that crime was "afoot").

70. See generally R. Richard Banks, *Beyond Profiling: Race, Policing and the War on Drugs*, 56 STAN. L. REV. 571 (2003); Samuel R. Gross & Debra Livingston, *Racial Profiling Under Attack*, 102 COLUM. L. REV. 1431 (2002); see also Kevin R. Johnson, *Racial Profiling After September 11: The Department of Justice's 2003 Guidelines*, 25 IMMIGR. & NAT'LITY L. REV. 85 (2004); Bernard Harcourt, *Against Prediction: Sentencing, Policing, and Punishing in an Actuarial Age*, (Univ. Chi. Pub. L. & Legal Theory, Working Paper No. 94, 2005). See generally Andrew Taslitz, *Racial Blindsight: The Absurdity of Color-Blind Criminal Justice*, 5 OHIO ST. CRIM. L. 1 (2007).

The Violent Crime Control Act of 1994 anticipated the racial disproportionalities in the new policing.⁷¹ Congress included a provision that allowed the U.S. Department of Justice to sue local law enforcement agencies when it observes a pattern or practice of systemic violations of people's rights.⁷² Litigation under "Section 14141" beginning in the 1990s identified both Fourth and Fifth Amendment violations in racial profiling, leading to court supervision of both state and local police agencies.⁷³ Consent decrees were approved by federal district courts in three jurisdictions in the 1990s,⁷⁴ imposing obligations on local police departments to reform policy and practice to remedy constitutional violations including race discrimination in both stops and the use of force.⁷⁵

The first consent decrees focused on police use of force in Pittsburgh and Stubenville, Ohio.⁷⁶ The first consent decree alleging racial profiling in a state police agency was formalized in late 1999 in New Jersey, citing constitutional violations in the selection of motorists for stops and searches on the New Jersey Turnpike.⁷⁷ Consent decrees in Maryland, New Jersey, and Los Angeles soon followed, each adding to a foundation of empirical evidence of racially selective police enforcement.⁷⁸ A 1999 investigation by the

71. Violent Crime Control and Law Enforcement Act of 1994, Pub. L. 103-322, 108 Stat 1796 (1994).

72. *Id.* at § 210401. Section 210401, codified as 42 U.S.C. § 14141, provides, in part, that "[i]t shall be unlawful for any governmental authority, or any agent thereof, or any person acting on behalf of a governmental authority, to engage in a pattern or practice of conduct by law enforcement officers . . . that deprives persons of rights, privileges, or immunities secured or protected by the Constitution or laws of the United States." 42 U.S.C. § 14141.

73. See Rachel Harmon, *Promoting Civil Rights Through Proactive Policing Reform*, 62 STAN. L. REV. 1, 10, 12-13 (2009).

74. See Debra Livingston, *Police Reform and the Department of Justice: An Essay on Accountability*, 2 BUFF. CRIM. L. REV. 817, 817, 818 n.4 (1999).

75. See *Conduct of Law Enforcement Agencies*, DEP'T OF JUST., <https://www.justice.gov/crt/conduct-law-enforcement-agencies> [<https://perma.cc/2V5U-25EH>].

76. See HUMAN RIGHTS WATCH, *SHIELDED FROM JUSTICE: POLICE BRUTALITY AND ACCOUNTABILITY IN THE UNITED STATES* 142-43 (1998) (discussing the first wave of consent decrees).

77. See generally *United States v. New Jersey*, No. 99-5970 (D.N.J. Dec. 30, 1999), <http://www.nj.gov/lps/jointapp.htm> [<https://perma.cc/5LYC-S2KV>].

78. See Mary D. Fan, *Panopticism for Police: Structural Reform, Bargaining and Police Regulation by Data Driven Surveillance*, 87 WASH. L. REV. 93, 94, 95 n.6 (2012); see also CHARLES EPP ET AL., *PULLED OVER* 52 (2014). See generally IAN AYRES & JONATHAN BOROWSKY, AM. CIV. LIBERTIES UNION OF S. CAL., *A STUDY OF RACIALLY DISPARATE OUTCOMES IN THE LOS ANGELES POLICE DEPARTMENT* (2008); Banks, *supra* note 70; Fagan & Davies, *supra* note 24; Joanna Schwartz, *Myths and Mechanics of Deterrence: The Role Of Lawsuits in Law Enforcement*

New York State Attorney General cited both Fourth and Fourteenth Amendment violations by the New York City Police Department in its conduct of *Terry* stops under its “stop and frisk” regime and a Stipulated Settlement in New York City.⁷⁹ In 2001, the Los Angeles Police Department finalized a consent decree⁸⁰ to remedy a pattern of “false arrests, using excessive force, conducting stops without reasonable suspicion, and . . . improper searches and seizures.”⁸¹ Between 2002 and 2014, consent decrees were implemented in fifteen cities across the country, revealing a set of systemic concerns about both the Fourth Amendment core of *Terry* and the more hidden Fourteenth Amendment prong.⁸²

Controversies over the racial prong of *Terry*’s “reasonable suspicion” standard arose shortly after *Terry* in a case where the Supreme Court justified the use of Mexican or Latino ethnicity to sanction police stop authority near the U.S.-Mexico border.⁸³ That case, in conjunction with *United States v. Brignoni-Ponce*,⁸⁴ a case decided a year earlier, are the only U.S. Supreme Court cases to specifically sanction the use of race or ethnicity in the decision to stop an individual under the Fourth Amendment rules stated in *Terry*.⁸⁵ A similar logic of profiling based on race and ethnicity was internalized in the early 1980s in drug enforcement training that led to racial disproportionality in highway stops.⁸⁶

Decisionmaking, 57 U.C.L.A. L. REV. 1023 (2010); Harcourt, *supra* note 70; Harris, *supra* note 31.

79. Eliot Spitzer, NEW YORK CITY POLICE DEPARTMENT’S “STOP & FRISK” PRACTICES: A REPORT TO THE PEOPLE OF THE STATE OF NEW YORK FROM THE OFFICE OF THE ATTORNEY GENERAL 148–49, 160–62 (1999); Andrew Gelman, Jeffrey Fagan & Alex Kiss, *An Analysis of the New York City Police Department’s “Stop-and-Frisk” Policy in the Context of Claims of Racial Bias*, 107 J. AM. STAT. ASS’N 813, 813 (2007). Those violations persisted through the Daniels stipulated settlement and up to Floyd verdict and order in 2013. See *Floyd v. City of New York*, 959 F. Supp. 2d 540, 633 (S.D.N.Y. 2013).

80. *United States v. Los Angeles*, No. 00-11769 GAF (C.D. Cal. June 15, 2001), http://www.lapdonline.org/assets/pdf/final_consent_decree.pdf [<https://perma.cc/D5YD-7CJ8>].

81. Letter from Bill Lann Lee, Acting Assistant, Att’y Gen., to James K. Hahn, City Attorney, City of Los Angeles (May 8, 2000) (notice of investigation letter to the Los Angeles Police Department), <http://www.justice.gov/crt/about/spl/documents/lapdnotti.php> [<https://perma.cc/N4EB-VE9K>].

82. *Conduct of Law Enforcement Agencies*, *supra* note 75.

83. *United States v. Martinez-Fuerte*, 428 U.S. 543 (1976).

84. *United States v. Brignoni-Ponce*, 422 U.S. 873 (1975).

85. See *Martinez-Fuerte*, 428 U.S. at 563; *Brignoni-Ponce*, 422 U.S. at 885–86.

86. See Harris, *supra* note 31, at 278–79; see also Gross & Livingston, *supra* note 70, at 1431.

In 1996, the Supreme Court sanctioned pretextual rationales motivating automobile stops in *Whren v. United States*.⁸⁷ While not explicitly a rationale for the use of race in selective enforcement, the Court refused to consider any factors other than objective considerations that animated the officer's actions.⁸⁸ In other words, having satisfied probable cause requirements under the Fourth Amendment, a search motivated by race would not pose a constitutional violation.⁸⁹ *Whren* effectively separated Fourteenth Amendment equal protection considerations from Fourth Amendment protections against unreasonable searches,⁹⁰ a separation that was repaired in *Floyd v. City of New York* in 2013.⁹¹

While maintaining its ban on the explicit use of race as an objective factor to justify stops, the Supreme Court expanded its analysis of race and policing in *Illinois v. Wardlow* to permit police stops based in part on robust correlates of race.⁹² The Court noted that, although an individual's presence in a "high crime area" does not meet the standard for a particularized suspicion of criminal activity, a location's characteristics are relevant to determining whether a behavior is sufficiently suspicious to warrant further investigation.⁹³ But the *Wardlow* court offered little guidance to police and courts to define a

87. 517 U.S. 806, 813 (1996) (quoting *Scott v. United States*, 436 U.S. 128, 138 (1978) ("[T]he fact that the officer does not have the state of mind which is hypothecated by the reasons which provide the legal justification for the officer's action does not invalidate that action . . .")).

88. *Id.* In the same paragraph, the *Whren* Court says that previous holdings show that the only time when an officer's motive invalidates objectively justifiable behavior, including searches, is in the context of an inventory or administrative search.

89. Kevin R. Johnson, *The Story of Whren v. United States: The Song Remains the Same*, in RACE AND LAW STORIES 419, 435 (Devon Carbado & Rachel F. Moran eds., 2006).

90. *See id.*; see also Bernard E. Harcourt, *United States v. Brignoni-Ponce and United States v. Martinez-Fuerte: The Road to Racial Profiling*, in CRIMINAL PROCEDURE STORIES 315 (C. Steiker ed., 2006).

91. *Floyd v. City of New York*, 959 F. Supp. 2d 540, 633 (S.D.N.Y. 2013) (holding that "McDonald was stopped, in violation of the Fourth and Fourteenth Amendments, because he was a black man crossing the street late at night in Queens").

92. *See Illinois v. Wardlow*, 528 U.S. 119, 124 (2000) ("An individual's presence in an area of expected criminal activity, standing alone, is not enough to support a reasonable, particularized suspicion that the person is committing a crime. But officers are not required to ignore the relevant characteristics of a location in determining whether the circumstances are sufficiently suspicious to warrant further investigation.").

93. *Id.* (citing *Adams v. Williams*, 407 U.S. 143, 144, 147–48 (1972)) ("Accordingly, we have previously noted the fact that the stop occurred in a 'high crime area' among the relevant contextual considerations in a *Terry* analysis.").

“high crime area,” in terms of its boundaries, crime levels, duration of crime, type of crime, or whether the standard varies across cities or parts of the country.⁹⁴ In effect, the standard was subjective and in turn, beyond regulation.⁹⁵ The *Wardlow* court left it up to the judgment not just of police officials but also of the individual officer to determine whether the location where she encountered a person whose behavior attracts her gaze meets the definition of “high crime area.”⁹⁶ While celebrating local knowledge, much the same as the *Terry* court celebrated Officer McFadden’s local knowledge and experience, the Court also complicated the design of standards that could be applied to audit and regulate its application.⁹⁷ In other words, *Wardlow* created a vague and subjective standard that would be difficult to regulate either institutionally or by courts. Since “high crime areas” and social disadvantage often are conflated both perceptually and statistically with concentrations of minority citizens,⁹⁸ this logic places minority neighborhoods at risk for elevating the suspiciousness of their residents in the eyes of the police.

94. See Andrew Guthrie Ferguson et al., *The “High-Crime Area” Question: Requiring Verifiable and Quantifiable Evidence for Fourth Amendment Reasonable Suspicion Analysis*, 57 AM. U. L. REV. 1587, 1605 (2008).

95. See *id.* at 1593, 1642.

96. See *Wardlow*, 528 U.S. at 124 (holding that “officers are not required to ignore the relevant characteristics of a location in determining whether the circumstances are sufficiently suspicious to warrant further investigation. Accordingly, we have previously noted the fact that the stop occurred in a ‘high crime area’ among the relevant contextual considerations in a *Terry* analysis.”).

97. See *id.* at 124 (“In this case, moreover, it was not merely respondent’s presence in an area of heavy narcotics trafficking that aroused the officers’ suspicion, but his unprovoked flight upon noticing the police. Our cases have also recognized that nervous, evasive behavior is a pertinent factor in determining reasonable suspicion.”); see also Ferguson et al., *supra* note 94, at 1594 (“After the Supreme Court’s decision . . . , the totality-of-the-circumstances test has devolved into a test that is met with two factors: high-crime area and unprovoked flight from police.”).

98. See generally GLENN LOURY, *THE ANATOMY OF RACIAL EQUALITY* (2002); DOUGLAS MASSEY, *CATEGORICALLY UNEQUAL: THE AMERICAN STRATIFICATION SYSTEM* (2007); DOUGLAS MASSEY & NANCY DENTON, *AMERICAN APARTHEID: SEGREGATION AND THE MAKING OF THE UNDERCLASS* (1993); Robert J. Sampson & Stephen W. Raudenbush, *Systematic Social Observation of Public Spaces: A New Look at Disorder in Urban Neighborhoods*, 105 AM. J. SOC. 603, 609–20, 622–30 (1999); Robert J. Sampson & Stephen W. Raudenbush, *Seeing Disorder: Neighborhood Stigma and the Social Construction of “Broken Windows,”* 67 SOC. PSYCHOL. Q. 319, 320–21 (2004); Anthony Thompson, *Stopping the Usual Suspects: Race and the Fourth Amendment*, 74 N.Y.U. L. REV. 956, 987–88 (1999); Jeffrey Fagan, *Crime and Neighborhood Change*, in UNDERSTANDING CRIME TRENDS 81 (A. Goldberger & R. Rosenfeld eds., 2008); Geoffrey Alpert et al., *Police Suspicion and Discretionary Decision Making During Citizen Stops*, 43 CRIMINOLOGY 407, 411–13 (2005).

B. Normative Tensions

The basis for targeted enforcement was an instrumental analysis of profiling as a tradeoff of harms that leads to a moral imperative to create wide space for police to act on race-based signals of suspicion.⁹⁹ Proponents of race-based selection of suspects, or racially disproportionate selection, based their preferences on three prongs: (a) higher crime rates among African American and Latino people (or in places where they are demographically concentrated), (b) the greater efficiency and effectiveness of police methods that apply race-based strategies, and (c) the moral imperative to pursue tactics that maximize social welfare and security.¹⁰⁰ To ignore race in the design of police tactics would be to risk greater exposure of individuals, including those in the affected groups, to unjustified harms.¹⁰¹ These utilitarian arguments for profiling assume that the social good produced by welfare outweighs the harms of the inequalities inherent in race-based selection of persons.¹⁰²

The arguments advancing profiling ran headlong into its constitutional weaknesses, even under a newly capacious Fourth Amendment suspicion standard that invited the substitution of race-based correlates of suspicion for explicit racial categories.¹⁰³ The

99. See Mathias Risse & Richard Zeckhauser, *Racial Profiling*, 32 PHIL. & PUB. AFF. 131, 131–35 (2004).

100. See *id.* at 135, 141–42, 151 (2004). But see Annabelle Lever, *Why Racial Profiling is Hard to Justify: A Response to Risse and Zeckhauser*, 33 PHIL. & PUB. AFF. 94, 109 (2005) (“[R]acial profiling will likely exacerbate the racism of a racist society and, at all events, will place particularly onerous burdens on those who are already burdened by racism. In short, the dangers of compounding injustices that are, already, grave and persistent, means that more is required by way of evidence and argument to justify racial profiling than to oppose it); Steven N. Durlauf, *Racial Profiling as a Public Policy Question: Efficiency, Equity, and Ambiguity*, 92 AM. ECON. REV. 132, 135(2005) (“First, . . . the current evidence on profiling and guilt rates does not represent the basis for a justification of current racial profiling practices. The effects of profiling on the crime rate are ambiguous, whereas there is a clear fairness violation involved. Second, there should exist a presumption against profiling policies which places the burden of proof on advocates of profiling to demonstrate that the efficiency effects are sufficient to overcome the fairness violation.”).

101. Risse & Zeckhauser, *supra* note 99, at 135.

102. A similar argument was made by Sunstein and Vermeule on capital punishment. They argued that if the death penalty was an effective deterrent to murder, then the life-life tradeoff of capital punishment created a more imperative to execute those convicted of capital murder, and a moral offense when executions are not carried out in the face of evidence of deterrence and the possibility of lives lost. See generally Cass Sunstein & Adrien Vermeule, *Is Capital Punishment Morally Required? Acts, Omissions, and Life-Life Tradeoffs*, 58 STAN. L. REV. 703 (2005).

103. See *Whren* and *Wardlow* discussions, *supra* Part I.A.

inherent constitutional violations deflated the moral arguments, as did the dubious claim of its effectiveness. But other critiques emerged that also pushed back on profiling.¹⁰⁴ Beyond the failure of proponents to find empirical support for the claim of its benefits, the utilitarian view tended to discount the serious harms to the innocent who are stopped, particularly innocent African Americans who bore the brunt of police actions.¹⁰⁵

Recognition of those harms was inherent in the *Terry* decision itself, which acknowledged that police stops, especially for the innocent, amounted to more than a “petty indignity.”¹⁰⁶ The harms of *Terry* stops are several: the stigma harm of being singled out when innocent, the shaming of being singled out by the police and physically interrogated in front of one’s family and neighbors, the racial stigma that attaches when minorities are disproportionately targeted for stops, and the potential for physical violence when stops arouse anger or when police use aggressiveness when confronted by their own safety fears.¹⁰⁷ Stops can be verbally harsh, physically aggressive, or laced with racial or sexual invectives that trigger a variety of emotional reactions.¹⁰⁸ Accordingly, a robust and consistent stream of research reveals numerous harms that people of color experience as a result of accumulated, proactive police contacts. Survey research in New York City under its stop and frisk program showed elevated rates of symptoms of post-traumatic stress disorder among the young adults most often stopped and most intrusively policed.¹⁰⁹

104. See generally Lever, *supra* note 100; see also Durlauf, *Racial Profiling as a Public Policy Question*, *supra* note 100.

105. Steven N. Durlauf, *Assessing Racial Profiling*, 116 ECON. J. 402, 420 (2006) (arguing that a Fairness Presumption would negate arguments for using race-based police stops as a public policy and practice).

106. See *Terry v. Ohio*, 392 U.S. 1, 16–17 (1968). “Moreover, it is simply fantastic to urge that such a procedure performed in public by a policeman while the citizen stands helpless, perhaps facing a wall with his hands raised, is a ‘petty indignity.’ It is a serious intrusion upon the sanctity of the person, which may inflict great indignity and arouse strong resentment, and it is not to be undertaken lightly.” *Id.*

107. William J. Stuntz, *Terry’s Impossibility*, 72 ST. JOHN’S L. REV. 1213, 1218 (1998); see also Bernard E. Harcourt, *Rethinking Racial Profiling: A Critique of the Economics, Civil Liberties, and Constitutional Literature, and of Criminal Profiling More Generally*, 71 U. CHI. L. REV. 1275, 1322 n.18 (2004). See generally Sherry F. Colb, *Innocence, Privacy, and Targeting in Fourth Amendment Jurisprudence*, 96 COLUM. L. REV. 1456 (1996); I. Bennett Capers, *Rethinking the Fourth Amendment: Race, Citizenship, and the Equality Principle*, 46 HARV. C.R.-C.L. L. REV. 10 (2010).

108. See Stuntz, *supra* note 107.

109. Amanda Geller et al., *Aggressive Policing and the Mental Health of Young Urban Men*, 104 AM. J. OF PUB. HEALTH 2321, 2326 (2014).

At the same time, a different form of equal protection complaints remained generally unheard and has only recently been taken seriously. Minority citizen concerns about the everyday under-policing of distressed neighborhoods also have received less scholarly attention.¹¹⁰ Nonetheless, police executives have increasingly come to understand that disadvantaged, high-crime minority communities may indeed simultaneously experience under- and over-policing.¹¹¹ Professor Randall Kennedy notes that “in terms of misery inflicted by direct criminal violence, blacks (and other people of color) suffer more from the criminal acts of their racial ‘brothers’ and ‘sisters’ than they do from the racist misconduct of White police officers.”¹¹² Given that both under- and over-policing have been shown to seriously undermine public confidence in and trust of the police, policy makers face a weighty challenge concerning how best to deliver effective crime control strategies without engaging in potentially racially discriminatory policing practices.¹¹³

C. Is It Bias? Approaches to Studying Police Stops and Searches

Recent empirical evidence on police stops supports perceptions among minority citizens that police disproportionately stop African American and Hispanic motorists, and that once stopped, these citizens are more likely to be searched or arrested.¹¹⁴ For example, surveys with nationwide probability samples, completed in 1999, 2002, and 2008 showed that African-Americans were far more likely than other Americans to report being stopped on the highways by police.¹¹⁵ Each survey showed that minority drivers also were more

110. *See generally* RANDALL KENNEDY, RACE, CRIME, AND THE LAW (1998).

111. *See id.*; *see also*, David C. Pyrooz et al., *Was There a Ferguson Effect on Crime Rates in Large U.S. Cities?*, 46 J. CRIM. JUST. 1 (2017).

112. KENNEDY, *supra* note 110, at 20.

113. *See id.*

114. *See generally* DAVID COLE, NO EQUAL JUSTICE: RACE AND CLASS IN THE AMERICAN CRIMINAL JUSTICE SYSTEM (1999); Ronald Weitzer & Steven A. Tuch, RACE AND POLICING IN AMERICA: CONFLICT AND REFORM (2006); *see also* MATTHEW H. ZINGRAFF ET AL., EVALUATING NORTH CAROLINA STATE HIGHWAY PATROL DATA: CITATIONS, WARNINGS, AND SEARCHES IN 1998 2 (2000); Harris, *supra* note 31, at 265–326; Samuel R. Gross & Katherine Y. Barnes, *Road Work: Racial Profiling and Drug Interdiction on the Highway*, 101 MICH. L. REV. 653, 660 (2002); Tom R. Tyler & Jeffrey Fagan, *Legitimacy, Compliance and Cooperation: Procedural Justice and Citizen Ties to the Law*, 6 OHIO ST J. CRIM. L. 231–75 (2008).

115. PATRICK LANGAN LAWRENCE ET AL., CONTACTS BETWEEN POLICE AND THE PUBLIC: FINDINGS FROM THE 1999 NATIONAL SURVEY 1 (2001); MATTHEW R. DUROSE, ERICA L. SCHMITT & PATRICK A. LANGAN, CONTACTS BETWEEN POLICE AND THE PUBLIC: FINDINGS FROM THE 2002 NATIONAL SURVEY iv (2005); CHRISTINE

likely to report being arrested, handcuffed, or searched by police, and that they more often were threatened with force or had force used against them.

Traffic violations also serve as the rationale or pretext for stops of motorists,¹¹⁶ just as “suspicious behavior” is the spark for both pedestrian and traffic stops.¹¹⁷ As with traffic violations, the range of suspicious behaviors is broad enough to challenge efforts to identify an appropriate baseline to which to compare race-specific stop rates.¹¹⁸ Pedestrian stops are at the very core of policing, used to enforce narcotics and weapons laws, to identify fugitives or other persons for whom warrants may be outstanding, to investigate reported crimes and “suspicious” behavior, and to improve community quality of life.¹¹⁹ Indeed, because low-level “quality of life” and misdemeanor offenses are more likely to be committed in the open, the “reasonable suspicion” standard is more easily satisfied in these sorts of crimes.¹²⁰

Two distinct approaches characterize recent efforts to model and understand racial disparities in police stops.¹²¹ Each focuses less on identifying racial bias than on understanding the role of race in explaining patterns of police behavior.¹²² Attributing bias is difficult: causal claims about discrimination would require far more information than the typical administrative (observational) datasets

EITH & MATTHEW. R. DUROSE, CONTACTS BETWEEN POLICE AND THE PUBLIC: FINDINGS FROM THE 2008 NATIONAL SURVEY 1 (2011).

116. Samuel Walker, *Searching for the Denominator: Problems with Police Traffic Stop Data and an Early Warning System Solution*, 3 JUST. RES. & POL'Y 63, 64 (2001); DAVID A. HARRIS, PROFILES IN INJUSTICE: WHY RACIAL PROFILING CANNOT WORK 30–33 (2002) (“[T]raffic stops offer a convenient way for police officers to sidestep restrictions on whom they may detain and serve as a powerful tool for putting criminal profiling into action.”).

117. See Alpert et al., *supra* note 98; see also AYRES & BOROWSKY, *supra* note 78, at 4.

118. Joel Miller, *Profiling Populations Available for Stops and Searches*, in POLICE RESEARCH SERIES PAPER 131, at 83 (*Home Office Research, Dev. & Statistics Directorate*, 2000), <http://webarchive.nationalarchives.gov.uk/20080305145815/http://www.homeoffice.gov.uk/rds/prgpdfs/prs131.pdf> [<https://perma.cc/U3VT-LFDW>]; Michael Smith & Geoffrey Alpert, *Searching for Direction: Courts, Social Science, and the Adjudication of Racial Profiling Claims*, 19 JUST. Q. 673, 673–704 (2002). See generally Jon Gould & Stephen Mastrofski, *supra* note 21.

119. Miller, *supra* note 118, at 10.

120. David Rudovsky, *Law Enforcement by Stereotypes and Serendipity: Racial Profiling and Stops and Searches Without Cause*, 3 U. PA. J. CONST. L. 296, 333 (2001); David Rudovsky, *Litigating Civil Rights Cases To Reform Racially Biased Criminal Justice Practices*, 39 COLUM. HUM. RTS. L. REV. 97, 108–09 (2007).

121. FAGAN ET AL., *supra* note 20, at 309, 315.

122. *Id.*

can supply. For example, when Officer McFadden stopped suspect John Terry, he used his law enforcement “experience” to interpret Terry’s behavior in front of the jewelry store.¹²³ The multiplicity of interacting factors complicated the identification of the role of race in the decision to detain Terry,¹²⁴ but several analyses of the facts and jurisprudence of *Terry* suggest that the Supreme Court opinion discounted the influence of race in the opinion.¹²⁵

In *Terry*, it would be difficult to identify race alone, apart from the context in which race was observed, as the factor that animated McFadden’s decision to stop and frisk suspect Terry.¹²⁶ Instead, reliable evidence of ethnic or racial bias in these instances would require experimental designs that control for these competing and interacting factors—situational context, demeanor of suspect—so as to isolate differences in outcomes that could only be attributed to race or ethnicity. Such experiments are routinely used in tests of discrimination in housing and employment.¹²⁷ But observational studies that lack such controls are often embarrassed by omitted variable biases: few studies can control for all the variables that police consider in deciding whether to stop or search someone, much less their several combinations or permutations. Research *in situ* that relies on direct observation of police behavior requires officers to articulate the reasons for their actions, a task that is vulnerable to numerous validity threats.¹²⁸ Sampling considerations, as well as the presence of researchers in the context of the decision, also challenge the validity of observational studies.

123. See *Terry v. Ohio*, 392 U.S. 1, 23, 33 (1968) (noting that “[i]t would have been poor police work indeed for an officer of 30 years’ experience in the detection of thievery from stores in this same neighborhood to have failed to investigate this behavior” and that Officer McFadden “had observed circumstances that would reasonably lead an experienced, prudent policeman to suspect that Terry was about to engage in burglary or robbery.”).

124. See generally RANDALL KENNEDY, *RACE, CRIME AND THE LAW* (1997).

125. Thompson, *supra* note 98, at 964 (“The Court’s legal analysis was almost entirely devoid of references to race.”); Rudovsky, *supra* note 120, at 109; see also Dorothy E. Roberts, *Race, Vagueness, and the Social Meaning of Order-Maintenance Policing*, 89 J. CRIM. L. & CRIMINOLOGY 775, 832 (1999). See generally Devon W. Carbado, *(E)Racing the Fourth Amendment*, 100 MICH. L. REV. 946 (2002); Devon W. Carbado & Mitu Gulati, *Working Identity*, 85 CORNELL L. REV. 1259 (2000).

126. Thompson, *supra* note 98, at 964.

127. DEVAH PAGER, *MARKED: RACE, CRIME, AND FINDING WORK IN AN ERA OF MASS INCARCERATION* 90 (2007); Devah Pager, *The Mark of a Criminal Record*, 108 AM. J. SOC. 937, 944 (2003).

128. See, e.g., Gould & Mastrofski, *supra* note 21, at 320. See generally, Alpert et al., *supra* note 98, at 320.

The first approach to studying racial disparities bypasses the question of whether police intend to discriminate on the basis of ethnicity or race, and instead focuses on disparate impacts of police stop strategies. This strategy is prevalent in studies of decisions in the context of highways stops.¹²⁹ In this approach, comparisons of “hit rates,” or efficiencies in the proportion of stops that yield positive results, serve as evidence of disparate impacts of police stops.¹³⁰ This type of analysis has been used in several studies, including studies of police behaviors on highways.¹³¹ This approach bypasses the supply-side question of who is stopped (and for what reason), and instead looks only at disparate impacts or outcomes for different groups.

Outcome tests are usually agnostic with respect to race-based motivations for stops or frisks versus a search for efficiency and deterrence.¹³² They can show when a particular policy or decision-making outcome has a disparate impact whose racial disproportionality is not justified by heightened institutional productivity, negating an efficiency rationale. In the context of profiling, outcome tests assume that the *ex post* probability that a police search will uncover drugs or other contraband is a function of the degree of probable cause that police use in deciding to stop and search a suspect.¹³³ If searches of minorities are less productive than searches of Whites, this could be evidence that police have a lower threshold of probable cause when searching minorities. At the very least, it is a sign of differential treatment of minorities that in turn produces a disparate impact.

Knowles, Persico, and Todd consider this “hit rate” approach theoretically as well as empirically in a study finding that, of the drivers on Interstate 95 in Maryland stopped by police on suspicion of drug trafficking, African Americans were as likely as Whites to have drugs in their cars.¹³⁴ Their theoretical analysis posits

129. See, e.g., Gould & Mastrofski, *supra* note 21, at 320.

130. STEFANIE SEQUINO & NANCY BROOKS, RACIAL/ETHNIC DISPARITIES IN TRAFFIC STOPS: ANALYSIS OF VERMONT STATE POLICE DATA, 2010–2011, 5 (2014), https://acluvt.org/issues/profiling/vsp_rpt_re-exam.pdf [https://perma.cc/75RH-Q9AD].

131. John Knowles, Nicola Persico & Petra Todd, *Racial Bias in Motor Vehicle Searches: Theory and Evidence*, 109 J. POL. ECON. 203, 204–05 (2001); Gross & Barnes, *supra* note 114, at 667; Ayres, *supra* note 25, at 133; see also Durlauf, *Assessing Racial Profiling*, *supra* note 105, at 403.

132. See generally IAN AYRES, PERVERSIVE PREJUDICE (2001); Jeffrey Dominitz & John Knowles, *Crime Minimization and Racial Bias: What Can We Learn from Police Search Data?*, 116 ECON. J. F368–F384 (2006).

133. Ayres, *Outcome Tests*, *supra* note 25.

134. Knowles, Persico & Todd, *supra* note 131.

a dynamic process that considers the behaviors of police and citizens of different races, and integrates their decisions in equilibrium where police calibrate their behavior to the probabilities of detecting illegal behavior, and citizens in different racial groups adjust their propensities to accommodate the likelihood of detection.¹³⁵ They concluded that the search for drugs was an efficient allocation of police resources, despite the disparate impacts of these stops on minority citizens.¹³⁶

Outcome tests can be constructed as quasi-experiments, with race as a treatment, to identify the role of race in the selection of citizens for searches. Ridgeway matched suspects within officers to compare the post-stop outcomes of White suspects to those of minority suspects in similar locations, stopped at similar times and for the same reasons.¹³⁷ He reports no differences in post-stop arrests (“hit rates”) despite the greater number of stops of non-Whites.¹³⁸ But this approach seeks to explain away contextual variables, especially neighborhood context, rather than explicitly incorporate these factors in an identification strategy. Close and Mason construct a disparate outcome quasi-experiment to identify the role of race in police searches by comparing the preferences of officers of different races to search motorists, controlling for the motorist’s race.¹³⁹ They use both an outcomes-based non-parametric (quasi-experimental) analysis and a standard benchmarking parametric (regression) approach, and report both personal biases and police cultural bias in their propensity to search African American and Latino drivers.¹⁴⁰

These are useful but limited strategies. The robustness of these designs is compromised, by the omission of several factors—some unobservable and others usually absent from administrative data—that might bias their claims, such as racial differences in the attributes

135. *Id.*

136. Ian Ayres, *Outcome Tests*, *supra* note 25; Samuel R. Gross and Katherine Y. Barnes, *Road Work: Racial Profiling and Drug Interdiction on the Highway* (2002); John Lamberth, *Report of John Lamberth, Ph.D.*, American Civil Liberties Union (1997), <http://www.clearinghouse.net/chDocs/public/PN-MD-0003-0006.pdf> [<https://perma.cc/V9FR-4C3X>].

137. Greg Ridgeway, *Analysis of Racial Disparities in the New York Police Department’s Stop, Question, and Frisk Practices*, RAND Corporation, TR-534 (2007), http://www.rand.org/pubs/technical_reports/2007/RAND_TR534.pdf [<https://perma.cc/T64F-F4BG>].

138. *Id.*

139. Billy R. Close & Patrick L. Mason, *Searching for Efficient Enforcement: Officer Characteristics and Racially-Biased Policing* 3 REV. L. & ECON. 263–321 (2007).

140. *Id.*

that police consider when deciding which motorists or pedestrians to stop, search or arrest,¹⁴¹ or differences in police behavior in neighborhoods or other social contexts with different racial makeup.¹⁴² The omission of neighborhood context also biases estimates of the proportionality of police stops of citizens. The randomizing equilibrium assumptions in the Knowles et al. approach—that both police and potential offenders adjust their behavior in response to the joint probabilities of carrying contraband and being stopped—tend to average across broad heterogeneous conditions both in police decision making and offenders' propensities to crime,¹⁴³ and discount the effects of race-specific sensitivities toward crime decisions under varying conditions of detection risk via police stop.¹⁴⁴ When these two concerns are addressed, Dharmapala and Ross identify different types of equilibria that lead to different conclusions about racial prejudice in police stops and searches.¹⁴⁵

Accordingly, the nature and extent of racial bias in the policing of motorists and pedestrians remains unsettled empirically.¹⁴⁶ Supply-side issues, both in the number and characteristics of the persons available for stops by virtue of law violation or even suspicious behavior, complicate the search game paradigm by skewing the

141. See, e.g., *Police Suspicion and Discretionary Decision Making During Citizen Stops*, 43 CRIMINOLOGY 407 (2005); Michael R. Smith, M. Makarios & Geoffrey P. Alpert, *Differential Suspicion: Theory Specification and Gender Effects in the Traffic Stop Context*, 23 JUST. Q. 271 (2006).

142. Alpert et al., *Police Suspicion*, *supra* note 98; Fagan & Davies, *Street Stops and Broken Windows*, *supra* note 24; Douglas A. Smith, *The Neighborhood Context of Police Behavior*, 8 CRIME & JUST. 313 (1986).

143. Durlauf, *Assessing Racial Profiling*, *supra* note 105, at 402; Dhammika Dharmapala & Stephen L. Ross (2004), *Racial Bias in Motor Vehicle Searches: Additional Theory and Evidence*, 3 CONTRIBUTIONS TO ECON. POL'Y & ANALYSIS, art. 12, [http://www.degruyter.com/dg/viewarticle.fullcontentlink:pdfeventlink/\\$002fj\\$002fbjeap.2003.3.issue-1\\$002fbjeap.2004.3.1.1310\\$002fbjeap.2004.3.1.1310.pdf?t:ac=j\\$002fbjeap.2003.3.issue-1\\$002fbjeap.2004.3.1.1310\\$002fbjeap.2004.3.1.1310.xml](http://www.degruyter.com/dg/viewarticle.fullcontentlink:pdfeventlink/$002fj$002fbjeap.2003.3.issue-1$002fbjeap.2004.3.1.1310$002fbjeap.2004.3.1.1310.pdf?t:ac=j$002fbjeap.2003.3.issue-1$002fbjeap.2004.3.1.1310$002fbjeap.2004.3.1.1310.xml) [https://perma.cc/9EPC-TK9B].

144. Jeff Dominitz & John Knowles, *Crime Minimization and Racial Bias: What Can We Learn From Police Search Data?* F368-F384 (Penn Inst. for Econ. Res., Working Paper No. 05-019, 2006).

145. Dharmapala & Ross, *supra* note 143.

146. Billy R. Close & Patrick Leon Mason, *Searching for Efficient Enforcement: Officer Characteristics and Racially Biased Policing*, 3(2) J. L. & ECON. 263, 263–321 (2007); Nicola Persico & Petra Todd, *Passenger Profiling, Imperfect Screening, and Airport Security*, AM. ECON. ASS'N PAPERS & PROCEEDINGS 127–31 (2009); Kate Antonovics & Brian G. Knight, *A New Look at Racial Profiling: Evidence from the Boston Police Department*, 91 REV. ECON. & STAT. 163–177 (2009); John Donohue & Steven Levitt, *The Impact of Race on Policing and Arrests*, 44 J. L. & ECON. 367–94 (2001); David Bjerck, *Racial Profiling, Statistical Discrimination, and the Effect of a Colorblind Policy on the Crime Rate*, 9 J. PUB. ECON. THEORY 543–67 (2007).

population of stopped drivers according to the *ex ante* probabilities of criminality that police officers assign to different racial groups. Institutional or individual differences in the goals of law enforcement may also create heterogeneity both in the selection of individuals to be stopped and the decisions to engage them in searches for drugs, weapons, or other contraband. Officers may pursue one set of law enforcement goals for one group—maximizing arrests—while pursuing a different set of goals—minimizing crime—for another. Racial nepotism or antagonism may lead to differences in police stop and search behaviors when officers of one race face choices as to stop or search a driver of the same or a different racial or ethnic group.¹⁴⁷

These complexities illustrate the difficulty of identifying the role of race in producing racial disparities in stops and searches, and suggest a second approach that incorporates the contexts in which individual officers consider race in their everyday interactions with citizens. Gelman et al. and Alpert et al. show how neighborhood context influences both the attribution of suspicion that animates an encounter and the outcomes of police-citizen encounters.¹⁴⁸ The institutional context of policing also may influence individual officers' decisions through stigmatizing neighborhoods as “high crime” or disorderly, skewing how officers perceive and interpret the actions of citizens. Institutional cultures also may implicitly tolerate such perceptual or cognitive schema and internalize them into policy preferences and strategic decisions, as well as internal preferences for reward, promotion, or discipline.

One advantage of this approach is that it can accommodate transparency, as the analyses in this Article show. When police scan for suspicious behavior, we have only vague ideas about how their discretion is managed, and even more vague ideas about what exactly it is that they are looking for.¹⁴⁹ While there may be nothing like an algorithm to explain how observations are formed, there at least are observable patterns. The worry in this regime is about race: unconscious patterns that shape the formation of suspicion based on archetypes such as the “symbolic assailant” and other processes that shape cognition and interpretation of behavioral cues. Transparency at least provides a window to observe what those processes produce.

147. Close & Mason, *supra* note 146.

148. Gelman, Fagan & Kiss *supra* note 79, at 813–23; Alpert et al., *Police Suspicion*, *supra* note 98.

149. William H. Simon, *In Defense of the Panopticon*, BOS. REV. (Sept./Oct. 2014).

In other words, it lets observers, assuming access to these records, observe the observers as they conduct surveillance.¹⁵⁰

What remains unknown in this process is the harms that may accrue from these routine invasions of privacy.¹⁵¹ The dignitarian concerns pose one type of harm: the fact that one is a target of surveillance signals to other observers and perhaps to the watching public the person is a potential threat. That alone can have stigmatizing consequences throughout the community of the observed.¹⁵² Apart from the dignity worries, conducting these observations and stops can have negative effects on the way that police regard citizens and respect their autonomy and privacy.¹⁵³

II. DATA AND METHODS

A. The Research Site

Boston, Massachusetts, is the site for this research. The FIO strategy in Boston encompasses street encounters of the sort envisioned in *Terry*, where officers temporarily detain and question persons for whom they have reasonable and articulable suspicion that “crime is afoot.”¹⁵⁴ This regime also includes non-contact observations, which we discussed earlier.

In this study, we analyzed data provided by the BPD on its FIO activity. The BPD Boston Regional Intelligence Center (BRIC) maintains an electronic database of FIO reports.¹⁵⁵ These forms are used to document BPD officer interactions with individuals suspected of criminal activity, or associates of those individuals, including direct

150. Worries about the panopticonistic approach range from anxiety about privacy and anonymity of individuals in their everyday movements, to anxiety about easy detection of and over-enforcement of low-stakes crimes, to the racial disparities that may be inherent in such regimes. DANIEL SOLOVE, NOTHING TO HIDE: THE FALSE TRADEOFF BETWEEN PRIVACY AND SECURITY (2011); I. Bennett Capers, *Rethinking the Fourth Amendment: Race, Citizenship, and the Equality Principle*, 46 HARV. C.R.-C.L. L. REV. 10–15 (2010).

151. Stuntz, *Fourth Amendment and Privacy*, *supra* note 67.

152. See, e.g., Jane Bambauer, *Hassle* 113 MICH. L. REVIEW 461, 461–585 (2015) (noting that when police conduct stops, the community watching those stops internalizes the stigma attached to the person who has been stopped. When no wrongdoing is found, the stigma may remain); see also Amanda B. Geller et al., *Aggressive Policing and the Mental Health of Young Urban Men*, 104 AM. J. PUB. HEALTH 2321(2014). There is no reason to believe that when police conduct such observations, a stigma signal may be produced even in the absence of any contact.

153. David Alan Sklansky, *Too Much Information: How Not to Think About Privacy and the Fourth Amendment*, 102 CAL. L. REV. 1069 (2014).

154. *Terry v. Ohio*, 392 U.S. 1, 30–31 (1968).

155. *Id.*

encounters and non-contact observations.¹⁵⁶ FIO reports are a central activity in the BPD's intelligence efforts to collect and analyze data on the activities and whereabouts of known and suspected criminals and their associates in Boston.¹⁵⁷ The reports document the name, date-of-birth, sex, and race of FIO subjects as well as the date, time, and location of interaction.¹⁵⁸

Our analysis focuses on the period from 2007 through 2010. During that time, BPD officers made N=204,739 FIO reports.¹⁵⁹ Compared to the residential population, the targets of FIO reports were disproportionately male, young, and Black.¹⁶⁰ For these 204,739 FIO reports, the subjects were 89.0% male, 54.7% ages 24 or younger, and 63.3% Black.¹⁶¹ According to the U.S. Census Bureau, in 2010, Boston had some 617,594 residents that were 47.9% male, 36.2% ages 24 or younger, and 25.1% Black.¹⁶²

At first glance, these differences are suggestive of racially disparate treatment in BPD FIO activity. However, these differences could also reflect crime risk differences in Boston's neighborhoods and population groups. Criminological research has long documented that criminal offenders are more likely to be young and male.¹⁶³ Violent crime problems also tend to concentrate in highly disadvantaged urban neighborhoods that are disproportionately populated by Black residents.¹⁶⁴

BPD officers are required to document the reason for the completion of each FIO report and required to note whether they

156. Boston Police Department Rules and Procedures, *Rule 323, Field Interrogation, Observation, Frisk, and/or Search Reports*, May 25, 2005, at 1.

157. *Id.*

158. *Id.*

159. *Id.*

160. *Id.*

161. *Id.*

162. *American Fact Finder*, U.S. CENSUS BUREAU <http://factfinder2.census.gov/faces/tableservices/jsf/pages/productview.xhtml?src=bkmk> [https://perma.cc/J5NP-AVSY] (accessed March 14, 2015).

163. David Farrington, *Age and Crime*, 7 CRIME & JUST. 189 (1986); Jeffery T. Ulmer & John H. Kramer, *The Interaction of Race, Gender, and Age in Criminal Sentencing: The Punishment Costs of Being Young, Black, and Male*, 36 CRIMINOLOGY 763–797 (1998).

164. Lauren J. Krivo, Ruth D. Peterson & Danielle C. Kuhl, *Segregation, Racial Structure, and Neighborhood Violent Crime*, 114 AM. J. SOC. 1765, 1765–802 (2009). “Concentrated disadvantage in urban neighborhoods, which are often populated by black residents, undermines local collective efficacy and gravely limits the ability of residents to address serious violent crime problems.” Anthony A. Braga & Rod K. Brunson, *The Police and Public Discourse on ‘Black-on-Black’ Violence*, NEW PERSPECTIVES IN POLICING, May 2015, at 8.

conducted *Terry* frisks for officer safety purposes and/or searches for the purposes of seizing evidence.¹⁶⁵ Some 40.5% of the FIO reports involve a frisk and/or search of the subject (82,919).¹⁶⁶ Officers have very limited space on the form to record their reasons for the FIO and, unfortunately, 75.0% (153,554) of the FIO reports simply state “investigation person” as the justification.¹⁶⁷ This lack of documentation of stop rationales prevents a Fourth Amendment analysis of the legal justifications for discretionary stops and searches of FIO subjects. Also, there is no information on the outcomes of the FIO events about whether the frisks and searches led to arrests, summons, or seizure of weapons or contraband. In fact, FIO events that did lead to either of those outcomes are not recorded. Officers default to the completion of an arrest report in those circumstances. In turn, the type of outcome analysis that was essential to resolving the Fourth and Fourteenth Amendment claims in the Floyd stop-and-frisk litigation in New York could not be completed here.

B. Empirical Strategy

Our empirical strategy combines two distinct approaches to estimate racial disparities. The first strategy is a disparate treatment strategy that examines stops in alternate empirical specifications looking first at aggregates—neighborhoods or police districts—and then individuals nested within those districts. We drew upon regression models developed by Fagan and colleagues¹⁶⁸ to investigate alleged violations of the Fourteenth Amendment to the U.S. Constitution by the New York City Police Department (“NYPD”) in their stop, question, and frisk (“SQF”) practices as part of the *David Floyd et al. v. City of New York et al.* U.S. District

165. Krivo, Peterson & Kuhl, *supra* note 164.

166. 38.6% of the FIO reports indicated that the subjects were frisked and 11.6% of the FIO reports indicated that the subjects were searched. All but 1.8% of the searches were reported in conjunction with a frisk of the subject. Moreover, descriptive statistical analyses revealed that the biggest differences between FIO type and subject race arose when the FIO involved a frisk and/or search relative to a more simple observation and/or interrogation. Some 29.5% of White subjects were frisked / searched during an FIO relative to the 45.4% of Black subjects, 40.5% of Hispanic subjects, and 35.6% of Asian or other race subjects. As such, FIO type was collapsed into two categories: 0 = No Search (Observed and/or Interrogated only) and 1 = Frisk and/or Search Conducted.

167. *Id.*

168. Report of Jeffrey Fagan, Ph.D. (2010) in *David Floyd et al. v. City of New York et al.*, U.S. District Court for the Southern District of New York, 08 Civ. 01034 (SAS), October 28; Gelman, Fagan & Kiss *supra* note 79.

Court complaint.¹⁶⁹ Their analyses examined whether the racial composition of NYPD precinct residents predicted stop patterns after controlling for the influences of crime, social conditions, and the allocation of police resources.¹⁷⁰ Here, we adapted their analytical framework to examine whether the racial composition of Boston neighborhoods predicts BPD FIO patterns, adjusting for crime, social and economic predictors, and police resources.

The second strategy exploits the availability of data on officer race to determine whether the observed differences in stop rates for White and non-White youths are a function of preference-based discrimination, or statistical discrimination. Statistical discrimination would reflect a tendency to stop one group at a higher rate than another based on observable characteristics such as known crime rates. But preference-based discrimination would reflect a tendency to prefer one group for stops over others based on factors unrelated to their observable differences in the targeted behavior.

A study by Professors Antonovics and Knight (AK) conducted this type of analysis in Boston based on traffic stops by the BPD. AK extended the Knowles, Persico and Todd (KPT)¹⁷¹ hit rate (or equilibrium) test to test for bias in traffic stops by Boston police officers from 2001–2003.¹⁷² Following the KPT equilibrium model, AK assume that officers will rationally stop motorists according to their beliefs about the comparative propensities to violate the law by carrying contraband—drugs or weapons.¹⁷³ If population groups understand the risks of violating the law, they will adjust their behaviors accordingly and their propensities should fall into equilibrium with other groups and with officers' preferences.¹⁷⁴ In a statistical model, differences in “hit rates” between groups should be

169. Second Amended Complaint, *David Floyd et al. v. City of New York et al.*, U.S. District Court for the Southern District of New York, 08 Civ. 01034 (SAS), October 28.

170. Report of Jeffrey Fagan, Ph.D., in *David Floyd et al. v. City of New York*, *supra* note 168; *Floyd v. City of New York*, Opinion and Order, 08 Civ. 1034 (SAS) (S.D.N.Y.), 58–60 (2013).

171. Knowles, Persico & Todd, *supra* note 134; *see also* Nicola Persico & Petra Todd, *Using Hit Rate Tests to Test for Racial Bias in Law Enforcement: Vehicle Searches in Wichita*, 515 ECON. J. 351 (2006). For a critique of the KPT equilibrium model, *see* David Bjerk, *Racial Profiling, Statistical Discrimination, and the Effect of a Colorblind Policy on the Crime Rate*, 9 J. OF PUB. ECON. THEORY 543 (2007).

172. Kate Antonovics & Brian G. Knight, *A New Look at Racial Profiling: Evidence from the Boston Police Department*, 91 REV. OF ECON. & STAT., 163–77 (2009).

173. *Id.*

174. *Id.*

negligible, a sign that equilibrium has been reached.¹⁷⁵ This form of statistical discrimination stands in contrast to preference based discrimination, where police will stop motorists based on preferences to detain or arrest citizens in a particular racial group independent from their assumptions about that group's propensity to carry contraband.

In this case, AK were able to observe the race of both the officer and the suspect and estimate the magnitude of preference-based discrimination. AK used officer race-suspect race dyads as a benchmark for discrimination, assuming that officers would be more likely to stop and search a suspect from a different racial group. Perhaps officers believe they are better able to detect signals of wrongdoing among persons within their own race, or same-group membership may lead to preferential treatment. Regardless of motive, evidence of higher rates of cross-race differences in search rates would suggest preference-based discrimination rather than simply statistical discrimination based on general beliefs about crime-propensity within each racial or ethnic group. Other studies have used the same strategy in different settings to reach the same conclusions.¹⁷⁶

However, the previous studies using officer-race benchmarks have examined bias in vehicle searches pursuant to traffic stops. In our strategy, we exploit the availability of these same data to estimate race-based preferences among officers in street stops, preferences that are not easily explained by assumptions about each group's propensity for crime.

175. Knowles, Persico & Todd, *supra* note 134. *But see* Bernard Harcourt, *Against Profiling*, *supra* note 116 (arguing that the assumptions of consistent responses—or elasticities—across different racial groups is unrealistic given their overall exposure to both legal earning and the potential punishment costs from detection).

176. Close & Mason, *supra* note 146; Jeffrey Fagan & Amanda B. Geller, *Profiling and Consent* (Colum. L. Sch., Working Paper, 2010), <http://ssrn.com/abstract=1641326> [<https://perma.cc/WL2D-ABGE>]; J. Mitchell Pickerill, Clayton Mosher & Travis Pratt, *Search And Seizure, Racial Profiling, and Traffic Stops: A Disparate Impact Framework*, 3 L. & POL'Y 1 (2009). *But see* Rob Tillyer, Charles F. Klahm & Robin S. Engel, *The Discretion to Search: A Multilevel Examination of Driver Demographics and Officer Characteristics*, 28 J. CONTEMP. CRIM. JUST. 184,184–205 (2012) (showing that suspect race effects are reduced after controlling for a set of contextual effects, but remain significant).

1. *Disparate Treatment by Race*

The general test for evidence of disparate treatment is a regression equation that takes the form:

$$\text{Outcome} = \alpha + \beta_1 * \text{Minority} + \sum \beta_i * (\text{Plausible non-Race Influences}) + \epsilon_i$$

where *Outcome* is the event or status of interest, *Minority* is an indicator for the racial composition or status of the unit observed (i.e., neighborhood or person, depending on the outcome), *Plausible Non-Race Influences* are a set of variables representing non-race factors that also might influence the outcome, and an error term that captures the variation in the outcome that cannot be explained by either Minority status or the Non-Race Influences. These models may include non-race influences that are correlated with race, so as to better identify the unique effects of race that are present once the influence of proxies for race are removed.¹⁷⁷ The goal in specifying these models is to identify the effects of race on outcomes after simultaneously considering factors that may be relevant to race.¹⁷⁸ Under a disparate treatment theory, the critical question is whether

177. For a general discussion of the specification of regression models to test for disparate treatment, see generally D. James Greiner, *Causal Inference in Civil Rights Litigation*, 122 HARV. L. REV. 533, 565–72 (2008) (arguing for an analytic approach simulating an experiment by searching for units (persons) that are similar to one another in all observable ways except treatment (in our case, suspect race), ignoring the data from units that have no counterparts or matches, and conducting separate regressions for the “treated” and “untreated groups”). However, balancing between “treated” and “untreated” groups is made more difficult and less reliable by the sorting processes of individuals to treatments, or by limitations in the number and types of covariates that explain such sorting, and the plausibility of such counterfactuals. In our case, people are not randomly “allocated” to race, nor are the factors that explain racial sorting by age and socio-economic status, as well as by neighborhood or other important context such as school randomly distributed. And, searching only for “matched” cases inevitably results in discarding data and loss of precision. One solution is separate regressions for each “treatment” condition to determine what *might* take place for cases that were excluded due to lack of plausible counterfactuals, and appending this information to the basic comparisons across groups. For a general discussion of how regressions specify these sorting mechanisms that influence predictors of an outcome, see Thomas J. Campbell, *Regression Analysis in Title VII Cases: Minimum Standards, Comparable Worth, and Other Issues Where Law and Statistics Meet*, 36 STAN. L. REV. 1299 (1984).

178. See, e.g., *Griggs v. Duke Power Co.*, 401 U.S. 424 (1971). In a disparate treatment claim, we would ask if the use of a high school diploma requirement biases the hiring process since African American job applicants may be less likely to have obtained a high school diploma. Once this race-correlated control is introduced, it would likely reduce the racial disparity in the hiring rates and provide a different test than would a simple disparate impact test.

an applicant's race was the cause of being denied employment. Failure to do so raises the risk of "omitted variable bias," which could lead to erroneous conclusions about the effects of variables that do appear in a regression test.¹⁷⁹

2. Measures and Model Specification

We analyze differences in stop rates by neighborhood to determine whether FIO activity is explained by local crime rates, or if there is additional variance that is explained by race. A race-neutral practice would predict a positive effect for local crime rates and non-significant effects for race once we control for crime.

The neighborhood analyses were conducted using 2010 U.S. Census tracts as the principal unit of analysis. Census tracts were used instead of BPD geographic units (e.g. districts, reporting areas) or smaller areal units (e.g. Census block groups, street segments). Tracts are areas roughly equivalent to neighborhoods developed by the U.S. Census Bureau for the purposes of analyzing populations.¹⁸⁰ According to the 2010 U.S. Census, Boston was comprised of N=181 tracts. Data on the social and economic conditions in these tracts were obtained from the 2007–2010 American Community Survey (ACS).¹⁸¹ Eight tracts were excluded from the analysis because there were no residents in these areas for a total N=173 tracts.¹⁸² The FIO data included date and geographical location (x-y coordinates) information that permitted aggregation of FIO counts to Census tracts and by differing time periods.¹⁸³ The main outcome variable was the monthly count of FIOs made in each Census tract between 2007 and 2010 (N=8,304; 173 Census tracts with 48 observations each).

179. See, e.g., Ian Ayres, *Three Tests for Measuring Unjustified Disparate Impacts in Organ Transplantation: The Problem of 'Included Variable' Bias*, 48 PERSP. BIOLOGY MED. 68 (2005); Ian Ayres, *Testing for Discrimination and the Problem of 'Included Variable Bias'* (Yale L. Sch., Working Paper, 2010), <https://perma.cc/G837-UPYY>.

180. *2010 Geographic Terms and Concepts—Census Tract*, U.S. CENSUS BUREAU https://www.census.gov/geo/reference/gtc/gtc_ct.html [<https://perma.cc/2KH5-SHWF>]; Nancy Krieger, *A Century of Census Tracts: Health and the Body Politic (1906–2006)*, 83(3) J. URB. HEALTH 355 (2006).

181. American Community Survey (ACS), U.S. Census Bureau, <http://www.census.gov/acs/> [<https://perma.cc/48SE-HGK3>].

182. These eight Census tracts included the Stony Brook reservation, Belle Isle Marsh reservation, the Harbor Islands, the Esplanade recreational area, the Franklin Park recreational area, and three commercial property waterfront areas.

183. 95.2% (194,858 of 204,739) of the FIO reports were geocoded to 2010 Census Tracts in Boston.

The specific estimation technique for this analysis, or the functional form of the regression equation, was responsive to the specific measure of FIO activity (monthly counts in Census tract units). Accordingly, models were estimated using negative binomial regressions. This class of regression models is appropriate for counts of events, such as FIO reports in a specific area, where assumptions about the independence of events cannot be reliably made.¹⁸⁴ These models also are appropriate for counts where the distribution are over-dispersed; that is, where the variance exceeds the sample mean. The model takes the form of:

$$p(y) = P(Y_i = y_i) = \frac{\Gamma(y_i + \phi)}{y_i! \Gamma(\phi)} \frac{\phi^\phi \lambda_i^{y_i}}{(\phi + \lambda_i)^{\phi - y_i}}$$

where Y_i is the expected count of events in each unit i given parameters that we observe. We estimate the incidence of events for overdispersed models as:

$$E[y_i | X_i, \varepsilon_i] = \exp[\alpha + \chi_i' \beta + \varepsilon_i] = h_i \lambda_i$$

where $h_i = \exp(\varepsilon_i)$ is assumed to have a one parameter gamma distribution, $G(\theta, \theta)$ with mean 1 and variance $1 / \theta = \kappa$.

We used a specific form of negative binomial regression known as General Estimating Equations (GEEs).¹⁸⁵ GEEs are beneficial for nested or hierarchically organized data, such as years within Census tracts, as they allow for the specification of within-subject correlations of observations. These nesting variables are treated as random effects in the estimating models. Random effects here include census tract correlations. To adjust for difference in population densities in the census tracts, we estimated population-averaged models. Since the analyses include a sequence of time periods (calendar months), the models include an AR(1) variance estimation function that adjusts for the serial autocorrelation (or autoregression) of the counts of events

184. Negative binomial regressions also are especially useful for discrete data such as event counts when the variance exceeds the mean across areas. JOSEPH M. HILBE, *NEGATIVE BINOMIAL REGRESSION* (2007); *see also* Richard Berk & John M. MacDonald, *Overdispersion and Poisson Regression*, 24 J. QUANT. CRIMINOLOGY 269 (2008); D. Wayne Osgood, *Poisson-Based Regression Analysis of Aggregate Crime Rates*, 16 J. QUANT. CRIMINOLOGY 21 (2000); David A. Freedman, *Statistical Models: Theory and Practice* (2005); WILLIAM GREENE, *ECONOMETRIC ANALYSIS* (2003).

185. JAMES W. HARDIN & JOSEPH M. HILBE, *GENERALIZED ESTIMATING EQUATION* (2003); Gary A. Ballinger, *Using Generalized Estimating Equations for Longitudinal Data Analysis*, 7 ORG. RES. METHODS 127 (2004).

within sampling units over long periods of time.¹⁸⁶ We controlled for yearly and seasonal variations¹⁸⁷ in the monthly counts of FIO reports by including fixed-effects for calendar quarter and year.¹⁸⁸

Police activity in Boston is closely linked to crime.¹⁸⁹ As such, we test whether crime rates in a neighborhood are linked to the intensity of BPD FIO activity in that area. We use crime incident data generated by the BPD on 113,419 “index” crime incidents in Boston between 2007 and 2010.¹⁹⁰ These crime incident data were geocoded, and then aggregated by Census tract and month of occurrence to create a covariate measuring lagged and logged monthly counts of serious crime in Boston census tracts.¹⁹¹ As Figure 1 reveals, FIO reports made by BPD officers in 2010 tended to concentrate in census tracts with higher rates of total crime incidents and higher percentages of black resident populations. Figure 1 also shows a high degree of spatial autocorrelation in the concentration of FIO reports

186. AR(1) adjustments reflect the reality that the best predictor of what the crime rate will be in the next month is what it was in last month. This is an empirical constraint in identifying the relationship between crime and policing. Failure to correct for this temporal dependence will bias the standard errors in estimates of crime effects on policing, and this distortion remains even when fixed effects are used to control for temporal trends. See BADI BALTAGI, *ECONOMETRIC ANALYSIS OF PANEL DATA* (2001); Badi Baltagi & Qi Li, *Testing AR(1) Against MA(1) Disturbances in an Error Component Model*, 68 J. OF ECONOMETRICS 133 (1995).

187. There is a long tradition of studies of the seasonality of crime and the theoretical explanations for why crime varies by season. See, e.g., John R. Hipp et al., *Crime of Opportunity or Crimes of Emotion? Testing Two Explanations of Seasonal Change in Crime*, 82 SOC. FORCES 1333 (2004).

188. We created indicator variables to account for seasonal variations by calendar quarter. Quarter 1 represented January, February, and March monthly FIO counts (1 = Yes, 0 = No). Quarter 2 represented April, May, and June monthly FIO counts (1 = Yes, 0 = No). Quarter 3 represented July, August, and September monthly FIO counts (1 = Yes, 0 = No). Quarter 4 represented October, November, and December monthly FIO counts (1 = Yes, 0 = No). Quarter 1 served as the reference category for the seasonal polychotomous dummy variable. We also created indicator variables for year to account for annual variations in the data.

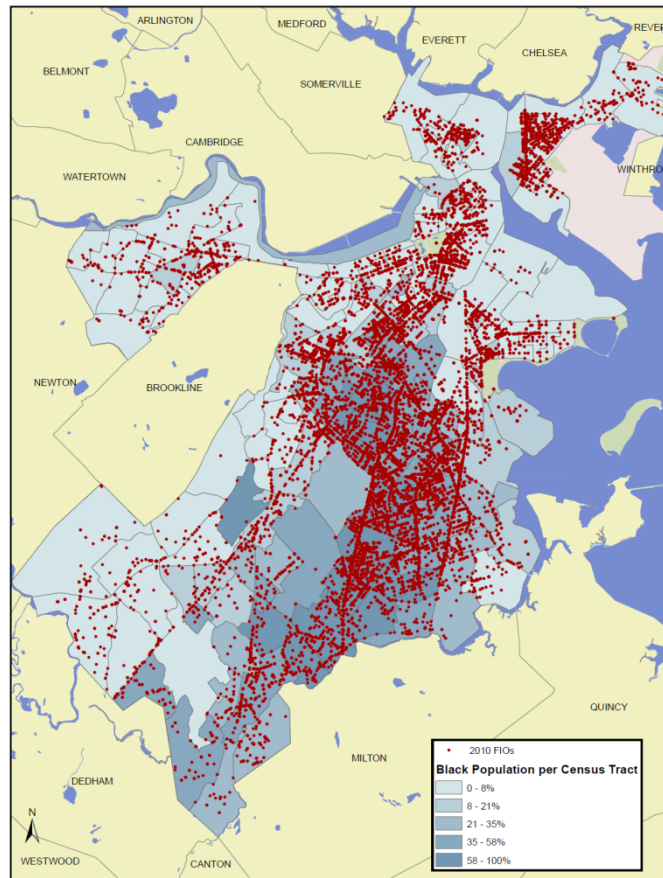
189. Anthony A. Braga et al., *An Ex-Post-Facto Evaluation Framework for Place-Based Police Interventions*, 35 EVALUATION REV. 592 (2011).

190. Index crimes, as defined by the FBI, included murder, rape, robbery, aggravated assault, burglary, auto theft, and larceny. See FBI, Uniform Crime Reporting, <http://www.fbi.gov/about-us/cjis/ucr> [<https://perma.cc/9JS2-5A78>]. Using ArcGIS 10.2 mapping software, the BRIC was able to geocode 113,152 of these incidents to their respective Census tracts (99.8 % of 113,419 total crime incidents).

191. All models control for the one-month-lag of logged total crime incidents. The natural log transformation of the actual number of crimes was used. Log transformation is necessary to adjust when the distributions are highly skewed and non-linear. The lag reflects the police planning process whereby FIO reports and other enforcement activity are adjusted to reflect actual crime conditions.

across Census tracts.¹⁹² We controlled for this spatial dependence in our regression models by including a Moran's I spatial effects covariate.¹⁹³

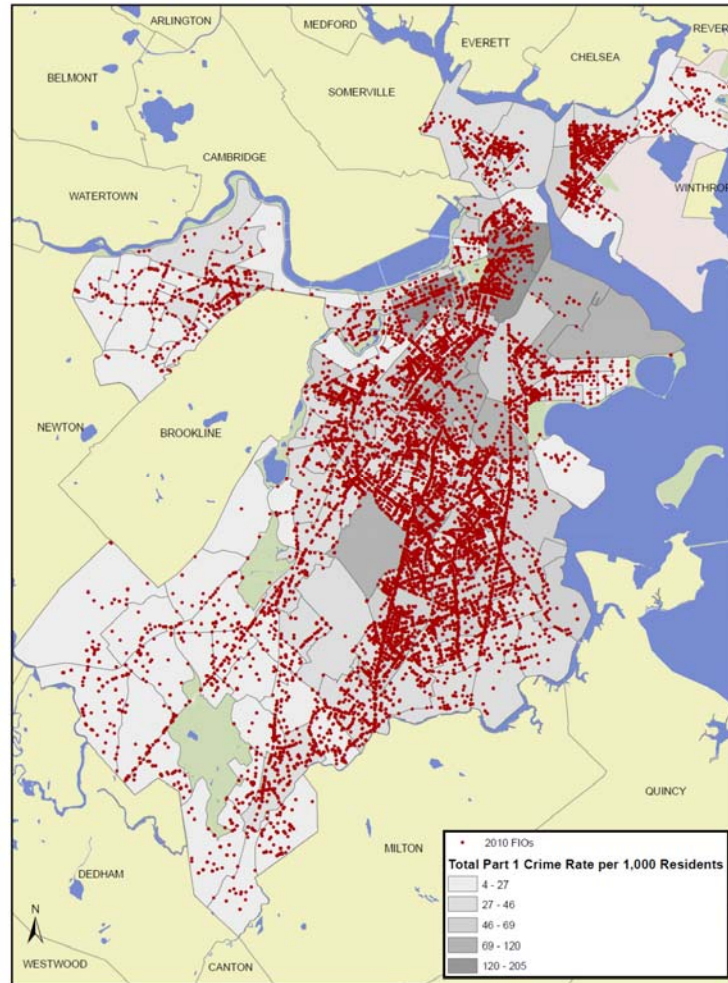
2010 Percent Black Population in Census Tracts



192. Spatial dependence, or autocorrelation, violates the assumption of independence among observations used in most statistical models. Spatial regression analyses of the variation of crime, etc., across neighborhood units account for spatial autocorrelation through the addition of a spatial effects covariate such as Moran's I. The argument is that analyses that do not compensate for spatial dependency can have unstable parameter estimates and yield unreliable significance tests. See MICHAEL D. WARD & KRISTIAN SKREDE GLEDITSCH, *SPATIAL REGRESSION MODELS* 8–10 (Sage U. Paper Series on Quantitative Applications in Soc. Sci., No. 155, 2008).

193. ArcGIS 10.2 was used to export a shapefile containing the total number of FIOs made per U.S. Census Tract during the study time period to GeoDa 1.4.6 spatial analysis software. Using queen's contiguity, a Moran's I = 0.674689 was estimated (199 permutations, $z = 14.73$, $p < .005$; 99 permutations, $z = 15.18$, $p < .01$). The Moran's I spatial autocorrelation lag for each Census Tract was exported to Stata 13.1 and included in the neighborhood analysis.

2010 Total Part 1 Crime Rate per 1,000 Census Tract Residents



We also control for police deployment patterns. The allocation of police and targeting of police activity frequently involved “saturation” deployment of police patrols in higher crime areas. Since these areas in Boston and elsewhere often had higher concentrations of non-White residents,¹⁹⁴ asymmetrical deployments of police increased exposure of citizens to police and thus the increased probability of encounters with minority citizens as

194. Ruth D. Peterson & Lauren J. Krivo, *Segregated Spatial Locations, Race-Ethnic Composition, and Neighborhood Violent Crime*, 623(1) AM. ACADEMY OF POL. & SOC. SCI. 93–107 (2009).

compared to Whites,¹⁹⁵ in turn producing racial or ethnic differences in contact patterns. Accordingly, an analysis of FIO patterns by neighborhood required an understanding of the allocation of police patrol resources in each unit of analysis. Patrol strength data were provided by the BPD for each of their eleven policing districts between 2007 and 2010. These patrol data were then allocated to each Boston census tract.¹⁹⁶

It is also important to note that the regulation and oversight of FIO policy and activities takes place at the police district level. There are 12 police districts in Boston, each commanded by a police captain who reports directly to the Superintendent of the Bureau of Field Services. BPD Captains are accountable for district-level crime trends and have discretion to allocate officers tactically within districts. Since tracts are nested within Boston's policing districts, we included fixed effects to account for any unobserved effects of conditions in the districts that might influence police activity, such as district-level variations in the use of FIOs to gather intelligence and maintain contact with potential offenders.¹⁹⁷

Several studies show that neighborhood crime rates, including violent crime,¹⁹⁸ are strongly associated with concentrated social

195. See, e.g., Donald Tomaskovic-Devey, Marcinda Mason & Matthew Zingraff, *Looking for the Driving While Black Phenomena: Conceptualizing Racial Bias Processes and their Associated Distributions*, 7 POLICE Q. 3 (2004).

196. Because BPD districts do not, as a rule, share boundaries with Census tracts, we allocated patrol strength to tracts based on the percent of each district's area that falls into each tract. For example, if Census tract A shares area with three police districts (A1, A2, and A3), the Census tract patrol strength was estimated as $[(\% \text{ of A1 falling into tract A} * \text{patrol strength of A1}) + (\% \text{ of A2 falling into tract A} * \text{patrol strength of A2}) + (\% \text{ of A3 falling into tract A} * \text{patrol strength of A3})]$.

197. The BPD has twelve districts that provide policing services across Boston's neighborhoods: A-1 serving Downtown, Beacon Hill, and Chinatown neighborhoods; A-15 serving Charlestown; A-7 serving East Boston; B-2 serving Roxbury and Mission Hill neighborhoods; B-3 serving Mattapan and parts of North Dorchester; C-6 serving South Boston; C-11 serving most of Dorchester; D-4 serving Back Bay, Fenway, and South End neighborhoods; D-14 serving Allston and Brighton neighborhoods; E-5 serving West Roxbury and Roslindale neighborhoods; E-13 serving Jamaica Plain; and E-18 serving Hyde Park. The reference category for the BPD district dummy variable was E-13. For a basic review of the use of dummy variables in regression models, see MELISSA A. HARDY, REGRESSION WITH DUMMY VARIABLES 7-16 (Sage U. Paper Series on Quantitative Applications in the Soc. Sci., No. 07-093, 1993).

198. Robert J. Sampson & William Julius Wilson, *Toward a Theory of Race, Crime, and Urban Inequality* in CRIME AND INEQUALITY 38 (John Hagan & Ruth Peterson eds., 1995); Robert J. Sampson, Steven Raudenbush & Felton Earls, *Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy*, 277 SCI. 918, 918 (1997); Jeffrey D. Morenoff, Robert J. Sampson & Steven Raudenbush,

disadvantage.¹⁹⁹ The concentrated disadvantage index is a standardized index composed of the percentage of residents who are Black, the percentage of residents receiving public assistance, the percentage of families living below the poverty line, the percentage of female-headed households with children under the age of 18, and the percentage of unemployed residents (as measured by the percentage of men over the age 16 who did not work in the previous year).²⁰⁰ Because we are explicitly interested the independent impact of race on the number of FIO reports in a neighborhood controlling for other factors, we excluded the percentage of Black residents from the construction of the Boston concentrated disadvantage used in this analysis. Because of the high correlation among these variables, we conducted principal components factor analysis to identify the underlying dimensions among the variables.²⁰¹ This procedure revealed that variables load on a single factor (which was retained as a standardized disadvantage index variable).²⁰² The presence of concentrations of recent immigrants is a protective factor that reduces the risk of crime in a neighborhood.²⁰³ As such, we created a variable that measured the percentage of foreign-born residents in each Census tract.

Neighborhood Inequality, Collective Efficacy, and the Spatial Dynamics of Urban Violence, 39 CRIMINOLOGY 517, 518 (2001).

199. Sampson & Wilson, *supra* note 198, at 45; Morenoff, Sampson & Raudenbush, *supra* note 198, at 518.

200. Morenoff, Sampson & Raudenbush, *supra* note 198, at 527.

201. Factor analysis is a statistical technique that captures consistency among observed variables to generate a composite measure using a lower number of unobserved variables. The method produces factors that represent the correlations among the observed measures. See JAE-ON KIM ET AL., FACTOR ANALYSIS: STATISTICAL METHODS AND PRACTICAL ISSUES at 8–9 (Sage U. Paper Series on Quantitative Applications in the Soc. Sci., No. 07-014, 1978). The principal components factor analysis was completed using STATA 13.1.

202. For example, a Boston Census tract featuring a disadvantage index score of 1.5 would be 1.5 standard deviations more disadvantaged than the mean Boston Census tract. As such, the disadvantage index is adjusted specifically for the city of Boston using 2010 ACS variables, even while the components used to construct the index remain constant across much neighborhood research and remain robust predictors of crime across a variety of city types and spatial aggregations. See Sampson, Raudenbush & Earls, *supra* note 198, at 920–21; Morenoff, Sampson & Raudenbush, *supra* note 198, at 543–44.

203. See, e.g., Robert J. Sampson, *Rethinking Crime and Immigration*, 7 CONTEXTS 28, 28–33 (2008), https://contexts.org/articles/files/2008/01/contexts_winter08_sampson.pdf [<https://perma.cc/XSM5-CZNC>].

3. Benchmarks

The selection of a benchmark against which to assess police enforcement activity is a basic question in reliably measuring the extent of racial disparities in police-citizen interactions.²⁰⁴ A benchmark allows us to determine if Boston Police are selectively, on the basis of race or another prohibited factor, singling out persons for FIO reports. As such, we compare the police decision to complete an FIO report on someone to their availability and eligibility for such reports, and compare that calculation across racial and ethnic groups. It is not hard to see that the reliability of an estimate of the extent of racial disproportionality or fairness is likely to depend on—and be particularly sensitive to—the benchmark used to measure criminal behavior.

Population is one measure of the supply of people available to the police for surveillance and possibly stops. However, there are constraints on local population estimates that limit its utility as a benchmark for the behavior of the police. Residential population estimates in commercial parts of Boston are often unreliable estimates of the actual composition of persons who are visible and available to the police during certain hours of the day. And, similarly, if people leave residential areas to work in commercial areas, the estimates in the residential areas will also be biased and inaccurate.

Another reason that population may not be an incomplete benchmark is that BPD officers do not complete FIO reports randomly based on the population parameters of an area.²⁰⁵ In fact,

204. The issues in benchmarking for pedestrian stops can be different from those that influence decisions on how to benchmark for traffic stops. See generally LORI A. FRIDELL, POLICE EXECUTIVE RESEARCH FORUM, BY THE NUMBERS: A GUIDE FOR ANALYZING DATA FROM VEHICLE STOPS (2004); Jeffrey Fagan, *Law, Social Science and Racial Profiling*, 4 JUST. RES. & POL'Y 104 (2002); Ian Ayres, *Outcome Tests of Racial Disparities in Police Practices*, 4 JUST. RES. & POL'Y 133 (2002); Greg Ridgeway & John MacDonald, *Methods for Assessing Racially Biased Policing, in RACE, ETHNICITY AND POLICING: ESSENTIAL READINGS* 180 (S.K. Rice & M.D. White eds., 2010). See also Samuel Walker, *Searching for the Denominator: Problems with Police Traffic Stop Data and an Early Warning Solution*, 4 JUST. RES. & POL'Y 133 (2002). The Fagan and Walker articles respectively wrestle with the unique demands of benchmarking for pedestrian stops (comparing the adequacy of benchmarks based on racial composition of local populations, the distribution of behaviors associated with criminal activity, the racial composition of known criminal offenders or suspects, local area crime rates, or comparisons of the behaviors of police officers of different races confronting suspects of different races).

205. See BOS. POLICE DEP'T, RULE 323: FIELD INTERACTION/OBSERVATION/ ENCOUNTER REPORT (FIOE REPORT) (2015), <http://static1.squarespace.com/static/5086f19ce4b0ad16ff15598d/t/56a2569205caa7ee9f29e6a2/1453479570208/rule323.pdf> [https://perma.cc/2RQZ-PQ86]; JEFFREY FAGAN ET AL., FINAL REPORT: AN

police complete FIO reports of persons based on, at least in theory, their perceptions of suspected crime, or their evaluation of citizen behaviors that may provide reasonable indicia of the potential that a crime has occurred or is about to take place.²⁰⁶ To the extent that rates of crime *suspicion* are correlated with rates of crime *commission*, observed crime rates are useful candidates to serve as a component of a benchmark.²⁰⁷

For this analysis of BPD FIO activity, a valid benchmark requires estimates of the supply of individuals of each racial or ethnic group who are engaged in the targeted behaviors and who are available to the police as targets of their stop authority and intelligence gathering activities. Since police often target resources to the places where crime rates and risks are highest, and where populations are highest,²⁰⁸ some measure of population that is conditioned on crime rates is an optimal candidate for inclusion as a benchmark.

The challenge in following this strategy is to identify a valid measure of crime. Ideally, we would include measures of the race-specific crime rates in each tract (or other social area) to help construct precise benchmarks based on the participation in the behavior of interest by persons of each race and ethnicity. However, there are practical problems in this approach. For example, many crimes are unreported to the police, and there are no valid victim surveys from which we can measure crime rates.²⁰⁹ There are similarly no surveys of self-reported crimes.²¹⁰ Race-specific arrest rates have been used as a proxy for race-specific crime rates, with a

ANALYSIS OF RACE AND ETHNICITY PATTERNS IN BOSTON POLICE DEPARTMENT FIELD INTERROGATION, OBSERVATION, FRISK, AND/OR SEARCH REPORTS 1–3 (2015).

206. Bernard E. Harcourt & Tracey L. Meares, *Randomization and the Fourth Amendment*, 78 U. CHI. L. REV. 809, 829 (2011).

207. Geoffrey Alpert et al., *supra* note 98, at 411–13.

208. *Id.*

209. Josine Junger-Tas & Ineke Haen Marshall, *The Self-Report Methodology in Crime Research*, 25 CRIME & JUST. 291, 292 (1999). However, the Bureau of Justice Statistics manages the National Crime Victimization Survey, “in which interviewed persons are asked about the number and characteristics of victimizations experienced during the prior 6 months” and extrapolates crime rates based off the information from the survey. BUREAU OF JUST. STAT., DATA COLLECTION: NATIONAL CRIME VICTIMIZATION SURVEY (NCVS) (2014), http://www.bjs.gov/index.cfm?ty=dc_detail&iid=245 [<https://perma.cc/U953-LYL3>]; Jennifer L. Truman & Lynn Langston, *Criminal Victimization, 2014*, DEP’T OF JUSTICE, Sept. 29, 2015, at 1, <http://www.bjs.gov/content/pub/pdf/cv14.pdf> [<https://perma.cc/6WAG-WGVF>]; LYNN LANGSTON ET AL., DEP’T OF JUSTICE, VICTIMIZATIONS NOT REPORTED TO THE POLICE 1 (2012).

210. *See also* Junger-Tas & Marshall, *supra* note 209, at 348–53 (assessing drawbacks of self-report studies).

lag function that reduces (but hardly eliminates) the problem of correlated error terms between current enforcement and past enforcement.²¹¹ However, there is strong disagreement about the validity of prior arrest rates, with some analysts offering positive rationales,²¹² while others have been critical.²¹³

An alternative measure is crimes reported to the police. However, crime reports do not provide a complete picture of the racial makeup of the offenders in those crimes.²¹⁴ While crime reports may provide a snapshot of the racial composition of those involved in crime commission, it is just that: a snapshot with only partial coverage of criminal activity. The data are further limited by the fact that many reported crimes lack a suspect identification or description.²¹⁵ Moreover, some types of suspected crimes that motivate FIO activity, such as weapons possession or drug possession, often do not follow from crime reports that identify the race of a suspect, so these base rates of offending are unknown.²¹⁶ Calls for service to the police are yet a third index, but 911 calls of this sort are difficult to apply to proactive patrol or the “new policing” given varying incidents of

211. See, e.g., FAGAN ET AL., *supra* note 20, at 309, 310; Gelman, Fagan & Kiss, *supra* note 79, at 813.

212. Expert Report of Dennis Smith at 22–26, *Floyd v. City of New York*, 959 F. Supp. 2d 540, No. 08 Civ. 01034 (S.D.N.Y. Nov. 15, 2010) (arguing that arrest statistics reveal patterns of crime suspects that can inform analyses of racial bias in police activity).

213. Arrest data incorporate information about crime patterns, but also contain uncertainty about unobservable components because of police decisions about allocating officers to specific places. See generally Greg Ridgeway & John MacDonald, *Methods for Assessing Racially Biased Policing*, in RACE, ETHNICITY AND POLICING: NEW AND ESSENTIAL READINGS 180 (S.K. Rice & M.D. White eds., 2010).

214. FED. BUREAU OF INVESTIGATION, UNIFORM CRIME REPORTS, CRIME IN THE UNITED STATES, 2014, <https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-43> [<https://perma.cc/N2PD-NUAB>] (noting that not all agencies provided ethnicity breakdowns of offenders); see generally WILLIAM BRATTON, N.Y. POLICE DEP’T, CRIME AND ENFORCEMENT ACTIVITY IN NEW YORK CITY (JAN. 1–DEC. 31, 2015), http://www.nyc.gov/html/nypd/downloads/pdf/analysis_and_planning/year_end_2015_enforcement_report.pdf [<https://perma.cc/T63L-PGSA>]; see also Dov Fox, *The Second Generation of Racial Profiling*, 38 AM. J. CRIM. L. 49, 49–50 (2010).

215. Andrew E. Taslitz, *Racial Auditors and the Fourth Amendment: Data with the Power to Inspire Political Action*, 66 L. & CONTEMP. PROBS. 221, 262 n.349 (2003).

216. Lesser included offenses are not always included on crime reports. However, the FBI does frequently collect lesser included offenses for its hate crime database. See FED. BUREAU OF INVESTIGATION, LAW ENF’T SUPPORT SECTION & CRIME STATISTICS MGMT. UNIT HATE CRIME DATA COLLECTION GUIDELINES AND TRAINING MANUAL 52 (2015), <https://www.fbi.gov/about-us/cjis/ucr/hate-crime-data-collection-guidelines-and-training-manual.pdf> [<https://perma.cc/7AAL-X8YH>].

mistaken reports and the heterogeneity of the purpose of the calls that include serious crimes, cats in trees, multiple reports of the same gunshot, domestic disturbances, or car break-ins.²¹⁷

To the extent that observed or reported crimes are leading indicators of those behaviors that are correlated with crime, crimes known to the police are important part of a valid benchmark. So too is population, as an index of the overall exposure of citizen as available targets for surveillance and interdiction. Accordingly, these analyses use both population and reported crime as benchmarks for understanding the racial distribution of FIO reports. Sensitivity tests applied alternate benchmarks including lagged race-specific arrest rates²¹⁸ and lagged race-specific suspect rates.²¹⁹ Natural log of the Census tract population, total number of arrested individuals in Census tract, and total number of suspects reported in Census tract were used as the offsets in the regression models.

These analyses were designed to test whether monthly counts of FIO reports in Census tracts were disproportionate to the racial composition of tract residents, racial composition of arrested suspects in the tract, and the racial composition of crime suspects as reported by victims in crime incident reports, after controlling for the known crime rate in the previous month and other characteristics that are correlated with crime. For each racial composition benchmark, three race categories (percent Black, percent Hispanic, and percent Asian or other) are included and the category of percent White is omitted.

217. Robin S. Engel, Michael R. Smith & Francis T. Cullen, *Race, Place, and Drug Enforcement*, 11 CRIMINOLOGY & PUB. POL'Y 603, 605 (2012) (claiming that 911 calls are a more robust and accurate measure of the relative crime problems in an area than are either arrests or reported crimes). *But see* Stephen D. Mastrofski, *Race, Policing, and Equity*, 11 CRIMINOLOGY & PUB. POL'Y 593, 597 (2012) (critiquing the use of 911 calls as a basis of suspicion to either allocate officers or justify the formation of suspicions necessary for a valid *Terry* stop or probable cause for a search or an arrest).

218. Between 2007 and 2010, the BPD arrested 28,427 suspects. The racial distribution of arrested suspects was as follows: 50.4% Black, 26.8% White, 20.6% Hispanic, and 2.2% Asian or other race category. Using ArcGIS 10.2 mapping software, the BRIC was able to geocode 24,590 of these arrests to their respective Census tracts (86.5% of 28,427 total arrests). While a 100% geocoding rate is always desired, the geocode rate in the current study exceeds the minimum acceptable threshold of 85%. *See* Jerry H. Ratcliffe, *Geocoding Crime and a First Estimate of a Minimum Acceptable Hit Rate*, 18 INT'L J. GEOGRAPHICAL INFO. SCI. 61, 61–72 (2004).

219. As described earlier, between 2007 and 2010, there were 113,419 Part I UCR crime incidents in Boston. Victims in these incidents reported information on 340,585 suspects. The racial distribution of these suspects was as follows: 41.2% Black, 21.8% White, 17.3% Hispanic, 2.0% Asian or other race category, and 17.7% unknown race.

This was done to avoid collinearity in the model estimation. As such, the coefficients for each racial group are based on comparison with the percent White of the benchmark in the tract. When a racial composition variable is significant, this means that its relationship to FIO activity is significantly different from that of the White racial composition of that benchmark in the Census tract. The parameter estimates were expressed as incidence rate ratios (i.e., exponentiated coefficients)²²⁰ and robust standard errors clustered by tracts were used.²²¹

III. RESULTS

A. Suspects and Officers

Table 1 shows the characteristics of both suspects and officers. Suspect identifiers were available for 199,331 (97.4% of 204,739) FIO encounters between 2007 and 2010.²²² From these, we were able to identify N = 72,619 unique subjects. Using gang intelligence databases maintained by BPD, we estimated that 5.5% (3967 of 72,619) of the suspects in FIO encounters were classified as gang members.²²³ The number of FIO's per suspect ranged from 1 to 249,

220. Incidence rate ratios are interpreted as the rate at which things occur; for example, an incidence rate ratio of 1.10 would suggest that, controlling for other independent variables, a one unit increase in the selected independent variable was associated with a ten percent increase in the rate at which the dependent variable occurs. See SOPHIA RABE-HESKETH & ANDERS SKRONDAL, MULTILEVEL AND LONGITUDINAL MODELING USING STATA, VOLUME II: CATEGORICAL RESPONSES, COUNTS AND SURVIVAL 374–76 (2005); see also KENNETH ROTHMAN & S. GREENLAND, MODERN EPIDEMIOLOGY 34–36 (2008).

221. Greg Ridgeway & John MacDonald, *Doubly Robust Internal Benchmarking and False Discovery Rates for Detecting Racial Bias in Police Stops*, 104 J. AM. STAT. ASS'N 661, 664 (2009); see also Gary King & Margaret E. Roberts, *How Robust Standard Errors Expose Methodological Problems They Do Not Fix, and What to Do About It*, 23 POL. ANALYSIS 159, 161–64 (2015).

222. FAGAN ET AL., *supra* note 205, at 5, 6 tbl. 1; see also CITY OF BOSTON, BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, <https://data.cityofboston.gov/Public-Safety/Boston-Police-Department-FIO/xmmk-i78r> [<https://perma.cc/RMT5-VCCP>] (for information about FIO encounters between 2011 and 2015).

223. See ANTHONY A. BRAGA, DAVID M. HUREAU, & LEIGH GROSSMAN, MANAGING THE GROUP VIOLENCE INTERVENTION: USING SHOOTING SCORECARDS TO TRACK GROUP VIOLENCE 15 (2014). The Boston Regional Intelligence Center (BRIC) created a classification system using several parameters to identify individuals as gang members. To be classified as a gang member by BRIC, a person has to accumulate ten points based upon the following criteria: prior validation by a BRIC-affiliated criminal justice agency that uses the same selection criteria (nine points), prior validation by a non-BRIC-affiliated criminal justice agency that uses similar selection criteria (eight points), self-admitted gang membership (eight points),

with an average of 2.74 FIO events per suspect, during the study period.²²⁴ About half (48.5%) had been arrested, with the number of arrests ranged from 1 to 63, with a mean of 5 arrests.²²⁵

use and/or possession of gang paraphernalia or identifiers (four points), gang-related photograph (two points), known gang tattoo or marking (eight points), information from reliable confidential informant (five points), information from anonymous source or tipster (one point), crime victim associated with rival gang (three or eight points depending on incarceration status), possession of gang documents such as by-laws (three or eight points depending on incarceration status), possession of gang publications (two points), participation in gang publication (eight points), possession of court and/or investigative documents involving an identified gang member (nine points), possession of printed or electronic media indicating membership (one point), contact with known gang members via FIO reports (two points per report), named in police incident report involving known gang member (four points per report), possession of gang membership material (nine points), information developed during surveillance and/or surveillance (five points), and other information (one point). *Id.* at 15 n.2.

224. FAGAN ET AL., *supra* note 205, at 5; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

225. FAGAN ET AL., *supra* note 205, at 5; *see also* Shima Baradaran & Frank L. McIntyre, *Predicting Violence*, 90 TEX. L. REV. 497, 536 (2012) (“A person’s number of previous arrests is a large predictor of future re-arrest; however, whether or not that prior arrest turned into a conviction is largely irrelevant as an additional predictor.”); Avinash Singh Bhati & Alex R. Piquero, *Estimating the Impact of Incarceration on Subsequent Offending Trajectories: Deterrent, Criminogenic, or Null Effect?*, 98 J. CRIM. L. & CRIMINOLOGY 207, 216 (2007) (“[P]rior arrests were associated with recidivism.”).

Table 1. Age, Gender, and Race of Unique BPD FIO Subjects and Officers				
	<i>FIO Subjects,</i> <i>N=72,619</i>		<i>FIO Officers,</i> <i>N=1750^a</i>	
	N	Percent	N	Percent
<i>Gender</i>				
Male	59,438	81.8	1,558	89
Female	13,181	18.2	192	11
<i>Age</i>				
Below 18	9,201	12.7	0	0
18–24	24,471	33.7	10	0.6
25–30	12,375	17	208	11.9
31–35	6,417	8.8	286	16.3
36–40	5,636	7.8	356	20.3
41–50	9,650	13.3	609	34.8
51 and older	4,869	6.7	281	16.1
Mean	29.2		41.3	
Median	26		41	
Range	12 to 71 years		23 to 64 years	
<i>Race</i>				
Black	30,849	42.5	418	23.9
White	25,758	35.5	1,139	65.1
Hispanic	9,693	13.3	150	8.6
Asian / Other	1,321	1.8	43	2.5
Unknown	4,998	6.9	0	0
<i>Selected Characteristics</i>				
Subjects				
Gang member	3,967	5.5		
Prior arrest (1+)	35,256	48.5		
Officers				
Gang Unit (YVSF)	65	3.7		
Detective (any rank)	212	12.1		
Patrol Officer	1,379	78.8		
Patrol Sergeant	130	7.4		
Patrol Lieutenant / Captain	23	1.3		
Dep. Supt. / Superintendent	6	0.3		
Note: a. These are the officers who have had one or more FIO encounter over the study interval.				

Most suspects were young: nearly half were younger than 25 years of age.²²⁶ One in three (33.7%) were between 18 and 24 years of age.²²⁷ Most were male (81.8%), consistent with known gender differences in crime rates by gender.²²⁸ Most suspects were Black (42.5%) or Hispanic (13.3%), each above their respective share of population in Boston in the 2010 census.²²⁹ Whites were underrepresented in the FIO subject pool relative to population share.²³⁰ As we discussed earlier, population is a weak benchmark, and we control for local crime rates in subsequent analyses.

About half of the FIO suspects (48.5%) had one or more prior arrests, and half did not.²³¹ To the extent that stops in general carry

226. FAGAN ET AL., *supra* note 205, at 5; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222; Bhati & Piquero, *supra* note 225, at 216 (“[A]ge was inversely associated with recidivism (older offenders were less likely to recidivate).”).

227. FAGAN ET AL., *supra* note 205, at 5.

228. *Id.*; *see also* UNIFORM CRIME REPORTS, *supra* note 228; Janet L. Lauritsen, Karen Heimer & James P. Lynch, *Trends in the Gender Gap in Violent Offending: New Evidence from the National Crime Victimization Survey*, 47 CRIMINOLOGY 361, 362 (2009); Bhati & Piquero, *supra* note 225, at 216–17.

229. FAGAN ET AL., *supra* note 205, at 5; U.S. CENSUS BUREAU, *2010 Census Interactive Population Search, Boston City, Massachusetts*, <http://www.census.gov/2010census/popmap/ipmtext.php?fl=25:2507000> [<https://perma.cc/ME59-MWY4>]; Bos. Police Dep’t, *Boston Police Commissioner Announces Field Interrogation and Observation (FIO) Study Results*, Oct. 8, 2014, <http://bpdnews.com/news/2014/10/8/boston-police-commissioner-announces-field-interrogation-and-observation-fio-study-results> [<https://perma.cc/R7EE-52JQ>] (“It also showed that Black subjects are 8% more likely to be stopped repeatedly and 12% more likely to be frisked and searched when controlling for other factors like Criminal History and Gang Membership in Violent Crime areas.”); Marina Carver, *Study Finds Boston Police Target African-Americans Disproportionately*, CNN (Oct. 9, 2014), <http://www.cnn.com/2014/10/09/us/boston-police-stop-frisk/> [<https://perma.cc/64V8-F7W4>]; Sebastian Murdock, *The Staggering Racial Disparity In Boston’s Police Encounters*, HUFFINGTON POST (Oct. 8, 2014), http://www.huffingtonpost.com/2014/10/08/stop-and-frisk-boston_n_5953688.html [<https://perma.cc/BD7S-W9HW>]. The American Civil Liberties Union also produced a report on the impact of BPD’s policing. *See* AM. CIV. LIBERTIES UNION, *BLACK, BROWN AND TARGETED 1* (2014), <https://aclum.org/app/uploads/2015/06/reports-black-brown-and-targeted.pdf> [<https://perma.cc/Q5K7-DQQT>]. *See also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 236.

230. FAGAN ET AL., *supra* note 205, at 5; U.S. CENSUS BUREAU, *2010 Census Interactive Population Search: Boston City, Massachusetts*, <http://www.census.gov/2010census/popmap/ipmtext.php?fl=25:2507000> [<https://perma.cc/5J3Z-W9YX>] (last visited Apr. 10, 2016); *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

231. Bhati & Piquero, *supra* note 225, at 216; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

risks of social and psychological harms,²³² the reach of FIOs to persons with no prior record extends an umbrella of suspicion to a group of primarily young people with no known criminal involvement.

Gangs are a focus of Boston police tactics.²³³ Yet few of the FIO suspects (5.5%) were known to the police as gang members.²³⁴ The department's gang unit was proportionately small, with 3.7% of the population of officers who were in the FIO database.²³⁵

BPD Officers were older,²³⁶ not surprisingly, but their age distribution suggests that they were experienced. More than half were over 40 years of age (50.9%), with a median age of 41.3 years.²³⁷ Nearly two officers in three were White (65.1%), and about one in four was Black (23.9%).²³⁸ Most were assigned to patrol commands, with about one in eight (12.1%) holding a detective's shield.²³⁹

232. Stuntz, *supra* note 67, at 1218; *see also* Ekow N. Yankah, *Policing Ourselves: A Republican Theory of Citizenship, Dignity and Policing—A Comment on Fagan 2–3* (Cardozo Legal Studies, Research Paper No. 400, 2013), <http://ssrn.com/abstract=2258048> [<https://perma.cc/385T-7A93>].

233. *See* BRAGA, HUREAU, & GROSSMAN, *supra* note 223, at 13–14.

234. FAGAN ET AL., *supra* note 205, at 7; BRAGA, HUREAU, & GROSSMAN, *supra* note 223, at 13–14. *But see* Boston Police Commissioner Announces Field Interrogation and Observation (FIO) Study Results, *supra* note 229 (“Gang Membership and prior arrest history are very strong predictors of repeated FIO’s.”); Bos. Police Dep’t, *Commissioner Evans Continues Efforts to Increase Transparency and Accountability of Policing Activities to the Public*, Jan. 8, 2016, <http://bpdnews.com/news/2016/1/7/commissioner-evans-continues-efforts-to-increase-transparency-and-accountability-of-policing-activities-to-the-public> [<https://perma.cc/5FFZ-XLSV>] (“[O]fficers were repeatedly stopping or observing individuals with criminal records and/or gang affiliation.”).

235. FAGAN ET AL., *supra* note 205, at 7; *see* Bos. Police Dep’t, *Operation Ceasefire*, <http://bpdnews.com/operation-ceasefire/> [<https://perma.cc/GY56-HZH8>]; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

236. FAGAN ET AL., *supra* note 205, at 7.

237. *Id.* Similarly, in 2010, the median age of the Boston police force was 44.32 years. BOS. POLICE DEP’T, ANNUAL REPORT 3 (2010), <http://static1.squarespace.com/static/5086f19ce4b0ad16ff15598d/t/511a8170e4b0d00cab69226e/1360691568147/Annual+Report+2010-small.pdf> [<https://perma.cc/U78V-DYNN>].

238. FAGAN ET AL., *supra* note 205, at 7; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222. Similarly, in its 2015 Workforce Report, the City indicated that about 66% of the Police Department was White, 23% was Black, 9% was Hispanic, and 2% was Other. CITY OF BOSTON, 2015 WORKFORCE REPORT 16 (2015), https://www.cityofboston.gov/images_documents/2015.04.14%20Final%20Draft-UPDATED_City%20of%20Boston%20Workforce%20Profile%20Report_tcm3-50873.pdf [<https://perma.cc/66PG-UCH7>].

239. FAGAN ET AL., *supra* note 205, at 7; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

The number of repeat FIO reports per subject is concentrated among a small number of individuals who experience large numbers of FIO encounters.²⁴⁰ Table 2 shows that about two FIO subjects in three (67.5%) experienced one FIO.²⁴¹ As a group, they accounted for 24.6% of the total number of FIO reports from 2007–2010.²⁴² About one in twenty (5.2%) experienced ten or more FIOs and, as a group, accounted for 40.2% of the total number of FIO reports made by BPD officers during this time.²⁴³

Table 2. FIO Report Distribution by Unique Subjects						
N of FIOs	N Subjects	% Subjects	Cum. % Subjects	Sum FIOs	% FIOs	Cum. % FIOs
51+	211	0.3	0.3	14,886	7.5	7.5
25–50	671	0.9	1.2	22,314	11.2	18.7
10–24	2,933	4	5.2	42,787	21.5	40.2
5–9	4,926	6.8	12	31,798	15.9	56.1
2–4	14,860	20.5	32.5	38,528	19.3	75.4
1 only	49,018	67.5	100	49,018	24.6	100
Total	72,619	100	100	199,331	100	100

FIO forms also report the badge numbers of the BPD officers who filled out the reports.²⁴⁴ Officer badge numbers were available for N=200,103 FIO reports (97.7% of 204,739).²⁴⁵ BPD personnel records

240. FAGAN ET AL., *supra* note 205, at 7, 7 tbl. 2; *Boston Police Commissioner Announces Field Interrogation and Observation (FIO) Study Results*, *supra* note 229; *Commissioner Evans Continues Efforts to Increase Transparency and Accountability of Policing Activities to the Public*, *supra* note 234.

241. FAGAN ET AL., *supra* note 205, at 7; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

242. FAGAN ET AL., *supra* note 205, at 7; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

243. FAGAN ET AL., *supra* note 205, at 7; *Boston Police Commissioner Announces Field Interrogation and Observation (FIO) Study Results*, *supra* note 229 (noting that “5% of the individuals FIO’ed account for more than 40% of the total FIO reports”); *Commissioner Evans Continues Efforts to Increase Transparency and Accountability of Policing Activities to the Public*, *supra* note 234.

244. FAGAN ET AL., *supra* note 205, at 8; *see also* AM. CIV. LIBERTIES UNION, *supra* note 229, at 7 (copy of a FIO report); *see also* BOS. POLICE DEP’T, RULE 323: FIELD INTERACTION/OBSERVATION/ENCOUNTER REPORT (FIOE REPORT) (2015), <http://static1.squarespace.com/static/5086f19ce4b0ad16ff15598d/t/56a2569205caa7ee9f29e6a2/1453479570208/rule323.pdf> [https://perma.cc/L9CZ-MXTA].

245. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

identified 2359 unique officers in its workforce between 2007 and 2010, including new hires and retirements during that time period.²⁴⁶ Personnel records were used to determine officer demographic information, years on the job, rank, assignment, and detective status for all sworn BPD officers. Badge numbers on FIO reports were used to identify the N=1,750 unique BPD officers.

Table 3. FIO Report Distribution by Unique BPD Officers						
N of FIOs	N Officers	% Officers	Cum. % Officers	Sum FIOs	% FIO	Cum. % FIO
1,000+	28	1.2	1.2	42,399	21.2	21.2
500 - 999	65	2.8	4	44,153	22.1	43.3
250 - 499	128	5.4	9.4	44,809	22.4	65.7
100 - 249	253	10.7	20.1	39,693	19.8	85.5
50 - 99	214	9.1	29.2	15,179	7.6	93.1
1 - 49	1,062	45	74.2	13,870	6.9	100
Zero	609	25.8	100	0	0	100
Total	2,359	100	100	200,103	100	100

About three officers in four (74.2% of 2359) made one or more FIO reports between 2007 and 2010.²⁴⁷ The counts ranged from 1 to 2315 FIOs.²⁴⁸ Officers averaged 84.3 FIOs over the four years, or 21 per year.²⁴⁹ Table 3 shows that, similar to the distribution of repeat FIOs among subjects, the number of repeat FIO reports per officer is also highly concentrated among a small number of individuals (Table 3).²⁵⁰ Nearly half (45.0%) generated fewer than 50 FIO reports and, as a group, accounted for 6.9% of the total number of FIO reports during the study time period.²⁵¹ A small group (4.0%, or approximately 70 officers) generated 500 or more FIOs; they

246. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

247. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

248. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

249. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

250. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

251. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

accounted for 43.3% of the total number of FIO reports made by BPD officers from 2007 to 2010.²⁵²

B. Race, Crime, and FIO's

1. *FIOs by Neighborhood Crime and Social Conditions*

Table 4 shows the results of the estimates of FIO activity using alternate benchmarks for racial composition. The monthly number of total Index crimes (logged, lagged) in a tract was a consistently significant positive predictor of the monthly count of FIO reports in a tract across models with varying benchmarks. This suggests that the intensity of BPD FIO activity in a tract is associated with the amount of serious crime experienced in a tract controlling for other conditions. An increase of 1% more total index crime incidents in the previous month leads to an increase of 10.6% (IRR=1.106) FIO reports in the following month. This is a large effect, considering that the average Boston census tract experiences 12.2 index crimes per month. Each of the models in Table 4 show that the Boston police prioritized crime problems in the allocation of FIO activity by tract and police district during this period.

252. FAGAN ET AL., *supra* note 205, at 8; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

Table 4. Negative Binomial Regressions of Monthly FIO Report Counts Controlling for Census Tract Characteristics, Crime, Police Activity, and Other Conditions for Three Racial Benchmarks (IRR, SE, <i>p</i>)			
	Residents	Arrestees	Crime Suspects
Percent Black	1.022 (.006) **	1.025 (.005) **	1.029 (.009) **
Percent Hispanic	1.041 (.008)**	1.016 (.008) *	1.040 (.011) **
Percent Asian / other	1.020 (.012)	0.917 (.052)	0.967 (.063)
Percent Unknown Race	----	----	0.922 (.015) **
Total Crime (logged, lagged)	1.106 (.026) **	1.125 (.036) **	1.091 (.027) **
Disadvantage Index	0.894 (.157)	0.911 (.178)	0.924 (.143)
Percent Foreign Born	1.016 (.009) +	1.017 (.007) *	1.019 (.009) *
Patrol Strength	1.006 (.006)	1.002 (.005)	1.006 (.006)
Moran's I (lagged)	1.285 (.369)	1.124 (.280)	1.054 (.282)
Constant	0.063 (.052) **	0.168 (.131) *	0.916 (.035) **
District Fixed Effects?	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes
Season Fixed Effects?	Yes	Yes	Yes
Standard Errors Clustered by Tract?	Yes	Yes	Yes
Observations	8,303	8,303	8,303
Groups	173	173	173
Wald Chi-Square	460.36	492.63	582.82
Wald degrees of freedom	25	25	26
Wald Chi-Square <i>p</i>	.000	.000	.000
Notes: Estimates reported as Incident Rate Ratios. Robust standard errors were clustered by census tract. Percent White is the reference category for the resident, arrestee, and suspect race dummy variables. The natural log of the total number of residents, total number of arrestees, and total number of suspects for each tract-month were used as exposure offsets in the respective regression models. Significance: + <i>p</i> < .10, * <i>p</i> < .05, ** <i>p</i> < .01			

After controlling for crime, Table 4 also shows that the racial composition variables for percent Black and percent Hispanic are positive and significant for all three models. The pattern of race effects suggests evidence of disparate treatment in FIO activity based on neighborhood racial composition. After controlling for local crime

rates, we observe higher rates of FIO activity for census tracts based on their Black or Hispanic racial composition, whether in residents, arrestees, or the race of known crime suspects. In each of these specifications, the percentage of Foreign Born Residents in a tract was also a statistically-significant predictor of increased FIO activity. Since foreign born residents of Boston are primarily persons of color, the focus of FIO activity in those neighborhoods reinforces the notion of disparate treatment by race and ethnicity.

The consistent size and direction of the race and ethnicity coefficients suggests a robust race effect controlling for crime, police activity, and other relevant factors, even if they were modest in size. Still, even modest effects can have practical significance. The disparity in the monthly count of FIO reports can be meaningful in census tracts with larger shares of minority residents, arrestees, and reported suspects. Using the residential racial composition variable as an example, the incidence rate ratio on percent Black suggests that a one-unit increase in the Black percentage of residents relative to the White percentage of residents in a Census tract is associated with a 2.2% increase (IRR=1.022) in the monthly count of FIO reports made by the BPD controlling for crime and other factors. The effects of race (and foreign born residents) in Table 4 were observed after controlling for the number of officers deployed in each police district, a measure of the exposure of local residents to police and their availability for FIO contacts.

Figure 2. Predicted Counts of FIO's per Month by Percent Black and Hispanic Residents in Tract, 2007–2010, Controlling for Crime & Social Conditions

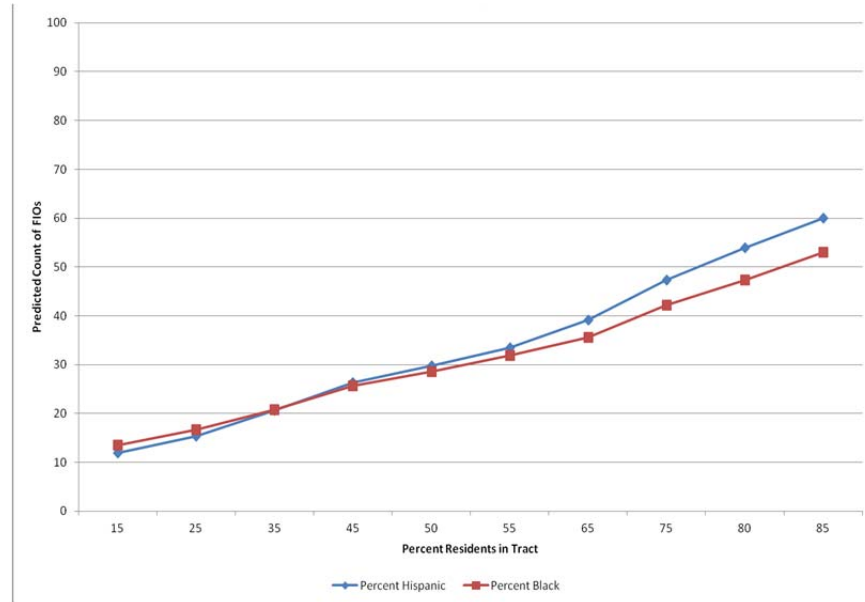


Figure 2 shows the marginal increase in the predicted count of monthly FIO reports in a census tract as the percentages of Black and Hispanic residents in a tract increase. The figure shows the nearly linear and monotonic increase in the adjusted (for predictors) monthly count of FIO reports increases as the percentages of minority residents increases in a tract. Simply to illustrate, Figure 2 shows that a tract with 85% Black residents would experience an additional 53 FIO reports per month compared to a tract with 15% Black residents.²⁵³ Over the course of one year, residents in that tract

253. FAGAN ET AL., *supra* note 205, at 9–10; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222. The trend of hyper-policing of majority Black neighborhoods can be observed in other cities as well. *See, e.g.,* AM. CIV. LIBERTIES UNION, STOP AND FRISK IN CHICAGO 3 (2015), http://www.aclu-il.org/wp-content/uploads/2015/03/ACLU_StopandFrisk_6.pdf [<https://perma.cc/QS3H-38KF>] (“Stop and frisk is disproportionately concentrated in the black community. Black Chicagoans were subjected to 72% of all stops, yet constitute just 32% of the city’s population.”); N.Y. CIV. LIBERTIES UNION, STOP & FRISK DURING THE BLOOMBERG ADMINISTRATION: 2002–2013 1 (2014), http://www.nyclu.org/files/publications/stopandfrisk_briefer_2002-2013_final.pdf [<https://perma.cc/C3KU-EV5A>] (“In 70 out of 76 precincts, black and Latino New Yorkers accounted for more than 50% of stops, and in 32 precincts they accounted for more than 90% of stops. In six of the 10 precincts with the lowest black and

would be subjected to an additional 636 FIO reports and, over the four-year study time period, this difference would represent an additional 2544 FIO reports in that tract.²⁵⁴

Because crime and racial composition are unevenly distributed across tracts and neighborhoods in Boston, similar to other cities, we tested for the possible leverage of outliers in the estimates in Table 4.²⁵⁵ That is, both of the central findings in Table 4 on crime and race could reflect the undue leverage and influence of neighborhood outliers in each of these distributions.²⁵⁶ For example, Figure 2 shows the concentration of crimes and race in particular corners of the city. To test for the effects of outliers, we conducted a sensitivity test by trimming 20% of tracts at the extremes of the FIO activity distributions. The results were largely unchanged. Using a population benchmark (Model 1 in Table 4), the IRR for percent Black population decline from 1.022 to 1.018 in the narrower model. For crime, the IRR of crime on FIO counts dropped from 1.106 to 1.088. In other words, the FIO / race / crime relationship is robust to the removal of the extremes.

2. FIO Activity by Suspect Characteristics

FIOs are a first-stage intrusion by police on individual liberty and privacy. But in Boston, the use of non-contact FIOs carries a lower level of intrusion. While privacy may be violated in the sense that one's movements in these contacts are recorded by a police officer acting on behalf of the state, a non-contact incident does not have the same physical intrusion nor temporary detention and liberty implications of a full contact stop.²⁵⁷ To compare race effects on contact versus non-contact encounters, we estimated negative binomial regressions of subject race and other individual characteristics on FIO counts. The models were estimated with and without gang membership status and arrest history to examine how

Latino populations (such as the 6th Precinct in Greenwich Village), blacks and Latinos accounted for about 70% or more of stops.”).

254. FAGAN ET AL., *supra* note 205, at 9–10; *see also* BOSTON POLICE DEPARTMENT FIO: BPD FIO FROM 2011 THRU JUNE 2015, *supra* note 222.

255. FAGAN ET AL., *supra* note 205, at 10; Krivo et al., *Segregation, Racial Structure, and Neighborhood Violent Crime*, *supra* note 24, at 1766–68.

256. For an example of an estimation of leverage effects of outliers, see Richard A. Berk, *New Claims About Executions and General Deterrence: Déjà Vu All Over Again?*, 2 J. EMPIRICAL LEGAL STUD. 303, 320–24 (2005) (showing the undue influence of Texas in state-year fixed effects estimates of the deterrent effects of executions on homicides).

257. FAGAN ET AL., *supra* note 205, at 11.

individual criminality might mediate any observed race effects. Model 1 in Table 5 shows the results for all FIO encounters. Model 2 controls for arrest history and gang membership, an adjustment that acknowledges the more intense surveillance and contact rates with suspected gang members or persons suspected by the police to be involved in criminal activity. Model 3 re-estimates Model 2 for only non-contact FIO encounters.

Table 5. Negative Binomial Regression of the Number of FIO Reports by Individual Suspect Characteristics Controlling for Gang Membership (IRR, SE, <i>p</i>)			
	<i>All FIO Reports</i>		<i>Non-Contact FIO Reports</i>
	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>
Black Suspect	1.725 (.026) **	1.088 (.011) **	1.047 (.010) **
Hispanic Suspect	1.136 (.026) **	0.969 (.013) *	0.972 (.012) *
Asian / Other Suspect	0.725 (.024) **	0.791 (.021) **	0.757 (.021) **
Unknown Race	0.501 (.007) **	0.681 (.007) **	0.483 (.007) **
Age	0.990 (.001) **	0.988 (.001) **	0.979 (.001) **
Female Suspect	0.670 (.011) **	0.830 (.009) **	0.811 (.008) **
Gang Member	----	3.339 (.076) **	4.171 (.075) **
Arrest History	----	1.108 (.001) **	1.151 (.001) **
Constant	2.788 (.058) **	2.103 (.029) **	2.091 (.029) **
District Fixed Effects?	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes
Season Fixed Effects?	Yes	Yes	Yes
SE's Clustered by Tract?	Yes	Yes	Yes
Observations	72,619	72,619	72,619
Log Pseudo-likelihood	-153,503.52	-133,092.42	-117,323.91
Wald Chi-Square	9,269.43	22,813.61	19,112.43
Wald Chi-Square <i>p</i>	0.000	0.000	0.000
Notes: Models estimated with robust standard errors clustered by tract. Race variables contrasted with White.			
Significance: + <i>p</i> <=.10, * <i>p</i> <=.05, ** <i>p</i> <=.01			

In Model 1, Black and Hispanic suspects have significantly higher FIO activity compared to Whites.²⁵⁸ The effect size for Blacks is especially large and more modest for Hispanic suspects.²⁵⁹ For Asian

258. *See supra* Table 5.

259. *See id.*

and Other Race suspects, they are less likely to be the subject of an FIO encounter compared to Whites, and the results also are significant.²⁶⁰ Older suspects and females are less likely to be subjects of FIO encounters.²⁶¹

Comparing Models 1 and 2, prior arrest history and gang membership each mediate the influence of race on the number of FIO encounters experienced by subjects, reducing the size of the race estimates but they remain statistically significant.²⁶² Model 1 shows that compared to White subjects, Black subjects experienced 72.5% more FIO encounters per month across the city and Hispanic subjects experienced 13.6% more FIO encounters.²⁶³ When the prior arrest and gang status covariates are included, in Model 2, Black subjects experienced only 8.8% more FIO encounters per month and Hispanic subjects experienced 3.1% fewer FIO encounters compared to their White counterparts.²⁶⁴ The results for Asians and Other or Unknown race suspects remains unchanged.²⁶⁵ Gangs evidently are a priority in using FIO authority, and account for at least some of the racial disparity in FIO encounters.

The pattern for non-contact FIO activity in Model 3 is similar to the pattern shown in Model 2.²⁶⁶ The effects of gang membership increase from Model 2 to Model 3, suggesting even greater attention to gang members, albeit without contact or interpersonal interaction.²⁶⁷ This makes sense, since gang members or reputed gang members are well known to the specialized Youth Violence Strike Force (YVSF, informally known as the gang unit), and their observations can be recorded for surveillance and intelligence purposes.²⁶⁸ Perhaps observing gang member movements and associations has intelligence payoffs, which might explain and rationalize the use of police powers in this way. Since there is a privacy but not liberty interest at stake in these non-contact encounters, there is little regulatory leverage in this practice.

260. *See id.*

261. *See id.*

262. *See id.*

263. *See id.*

264. *See id.*

265. *See id.*

266. *See id.*

267. *See id.*

268. Allison Manning, *Boston Police Data Shows Black Men Were Stopped Most Often*, BOSTON.COM (Jan. 19, 2016), <https://www.boston.com/news/local-news/2016/01/19/boston-police-data-shows-black-men-were-stopped-most-often> [https://perma.cc/J6KR-UZN8].

The importance of Table 5 is the finding suggesting intense police attention to gang members by the Boston police. Gangs are thought to be an important source of the city's gun violence problem, which leads to this attention, and gang membership also is skewed by both individual and neighborhood racial composition.²⁶⁹

3. *Frisks and Searches by Suspect Race*

Table 6 shows that Black and Hispanic suspects were more likely to be frisked or searched during an FIO encounter, after controlling for non-racial suspect characteristics.²⁷⁰ Compared to White suspects, Black suspects were 12.4% more likely to be frisked / searched, and Hispanic subjects were 4.5% more likely to be frisked / searched during FIO encounters with arrest and gang status covariates included in the model.²⁷¹ Gang members were 11.7% more likely to be frisked / searched during FIO encounters relative to their non-gang counterparts, controlling for other factors.²⁷² For every additional arrest in their history, suspects were 1.8% more likely to be frisked or searched during FIO encounters.²⁷³ Asian and other race subjects were significantly less likely to be frisked / searched during FIO encounters when compared to White subjects.²⁷⁴ Here, the gang effect that explained FIO activity in Table 5 seems to have comparable and independent influence on the decision to frisk as does the suspect's race.

269. See Anthony A. Braga, David M. Hureau & Andrew V. Papachristos, *Deterring Gang-Involved Gun Violence: Measuring the Impact of Boston's Operation Ceasefire on Street Gang Behavior*, 30 J. QUANTITATIVE CRIMINOLOGY 113, 118 (2013) [hereinafter *Deterring Gang Gun Violence*]; Andrew V. Papachristos, David M. Hureau & Anthony A. Braga, *The Corner and the Crew: The Influence of Geography and Social Networks on Gang Violence*, 78 AM. SOC. REV. 417, 426, 438 (2013) [hereinafter *The Corner and the Crew*]; Anthony A. Braga, David Hureau & Christopher Winship, *Losing Faith? Police, Black Churches and the Resurgence of Youth Violence in Boston*, 6 OHIO ST. J. CRIM. L. 141, 154 (2008) [hereinafter *Losing Faith?*].

270. See *infra* Table 6.

271. See *infra* Table 6.

272. See *infra* Table 6.

273. See *infra* Table 6.

274. See *infra* Table 6.

Table 6. Hierarchical Logistic Regression Estimating Impact of Suspect Race on Probability of a Frisk and/or Search (OR, SE, *p*)

<i>Characteristic</i>	OR	SE	<i>p</i>
Age	0.977	(.001)	**
Female	0.347	(.007)	**
Suspect Race–Black	1.124	(.018)	**
Suspect Race–Hispanic	1.045	(.018)	**
Suspect Race–Asian/Other	0.837	(.021)	**
Suspect Race–Unknown	0.588	(.018)	**
Gang Member	1.117	(.017)	**
Arrest History	1.018	(.001)	**
Constant	0.459	(.082)	***
Observations	199,331		
Log Likelihood	-121413.72		
Wald Chi-square	2603.82		
<i>p</i> (Wald Chi-square)	0.000		
Notes: Robust standard errors clustered by tract. Fixed effects for police districts, year and season. Random effects for tract characteristics (not shown) include tract population (logged), total violent crime in tract (logged, lagged), disadvantage index, and Moran's I. Race variables contrasted with White suspects.			
Significance: ⁺ <i>p</i> < .10, * <i>p</i> < .05, ** <i>p</i> < .01			

Taken together, Tables 5 and 6 show troubling racial disparities in the number of repeated FIO contacts and the probability of being frisked / searched experienced by Black and Hispanic suspects.²⁷⁵ The effects in these tables are adjusted for the influences of age, gang membership, neighborhood and other relevant non-race influences. In fact, we see the frisk estimates in Table 6 as conservative and expected to see even greater effects by suspect race considering the attention to gangs in this setting and BPD's use of FIOs for intelligence gathering purposes, especially among gang members. Other *Terry* stop “programs” do not document non-contact observations, in line with the Supreme Court dicta limiting constitutional regulation to the physical aspect of investigative stops.²⁷⁶ The large FIO differences in counts of encounters—both

275. See *supra* Table 5; see *supra* Table 6.

276. See generally *Terry v. Ohio*, 392 U.S. 1 (1968); see also Slobogin, *supra* note 44, at 22–23.

observational and face-to-face—compared to the incidence of frisks or searches suggests more extensive use of FIO reports to monitor gang members at a distance rather than repeatedly initiating physical contact to search them for weapons, drugs, or other contraband.²⁷⁷ Perhaps this is a safety consideration, or it may be that there are information yields from non-contact encounters, such as understanding gang membership and associations, that can address tactical and policy goals. Whatever the purpose and rational, more research is needed on the reasons and circumstances for this component of the FIO strategy, as well as its informational payoff.

4. *FIO Activity by Unit and Officer Race*

Table 7 shows the effects of officer characteristics on FIO patterns.²⁷⁸ There were large differences in FIO activity by officer race or ethnicity. Black officers made 42.5% fewer FIO reports per month compared to White officers, controlling for age, sex, rank, detective status, and assignment.²⁷⁹ Asian officers also made significantly fewer FIO reports.²⁸⁰ Relative to White officers, Asian officers made 44.8% fewer FIO reports, controlling for officer demographic, rank, and assignment covariates.²⁸¹ Hispanic officers made slightly smaller numbers of FIO reports than their White officers but the observed differences were not statistically significant.²⁸² Controlling for assignment, rank, and other factors, older officers and female officers made significantly fewer FIO reports relative to their younger and male counterparts, respectively.²⁸³

277. *See supra* Table 6.

278. *See infra* Table 7.

279. *See infra* Table 7.

280. *See infra* Table 7.

281. *See infra* Table 7.

282. *See infra* Table 7.

283. *See infra* Table 7.

Table 7. Zero Inflated Negative Binomial Regressions of FIO Counts on Officer Characteristics (IRR, SE, <i>p</i>)			
<i>Characteristic</i>	IRR	SE	<i>p</i>
Years on Job	0.902	(.007)	**
Female	0.377	(.069)	**
<i>Officer Race</i>			
Black	0.575	(.066)	**
Hispanic	0.901	(.156)	
Asian	0.552	(.121)	**
<i>Officer Rank</i>			
Detective	0.885	(.187)	
Sergeant or Lt.	0.893	(.151)	
Captain or Command	0.778	(.133)	*
<i>Officer Unit</i>			
Mobile Operations	1.021	(.583)	
Drug Control	1.131	(9.263)	
YVSF	11.953	(2.655)	**
Other Patrol	0.358	(.112)	**
Other Investigation	0.215	(.069)	**
Constant	206.322	(49.72)	**
<i>Zero Inflation Parameters</i>			
Administrative Assignment	4.946	(.404)	**
On Leave	4.592	(.389)	**
Constant	-4.734	(.301)	**
Observations	2,359		
Log Likelihood	-9,833.14		
Wald Chi-square	1059.06		
<i>p</i> (Chi-square)	0.000		
Notes: Models estimated with robust standard errors, not clustered due to mobility of officers. Fixed effects for police district, year, season, and police district.			
Significance: + $p < .10$, * $p < .05$, ** $p < .01$			

Unit assignment also was a significant predictor of officers' FIO activity. BPD officers assigned to the YVSF make almost 12 times as many FIO reports per month compared to officers assigned to other

specialized units or policing districts, controlling for other factors.²⁸⁴ Their mission explains in part this emphasis: YVSF officers are charged with preventing outbreaks of gang violence.²⁸⁵ Completing FIO reports on gang member whereabouts, their associations, and routine activities represent a central activity in pursuing that mission by massing information on the routine activities of gang members.²⁸⁶

Compared to line level patrol officers, Captains, Deputy Superintendents, and Superintendents make significantly fewer FIO reports holding other officer characteristics constant.²⁸⁷ These high-ranking officers have extensive managerial responsibilities and, while they maintain a presence in the community, they are much less likely to be engaging in street-level law enforcement work.²⁸⁸

The heavy influence of the YVSF officers on FIO activity, coupled with the race-specific patterns shown in Table 7, leads to a further question: whether FIO activity within the YVSF command also varies by officer race. Table 8 shows the results of regressions with only officers having one or more FIO encounters, and disaggregating officers by race and YVSF assignment.²⁸⁹ The six groups shown in Model 2 in Table 8 are compared to Asian and Other Race officers, a move that exploits the fact that there are so few Asian officers in the YVSF.²⁹⁰ This permits direct comparisons of the regression estimates in Model 2.

284. *See supra* Table 7.

285. *See* Manning, *supra* note 268.

286. *Id.*

287. *See supra* Table 7.

288. *See supra* Table 7. The model used for the estimates in Table 7 is a zero-inflated negative binomial regression, which is employed in situations where there are large numbers of observations of zero events in the data and there are separate functions to determine any participation and then frequency of participation. *See e.g.*, Kelvin K.W. Yau, Kui Wang & Andy H. Lee, *Zero-Inflated Negative Binomial Mixed Regression Modeling of Over-Dispersed Count Data with Extra Zeros*, 45 *BIOMETRICAL J.* 437 (2003). This regression first estimates factors that explain when there are one or more events, and then explains the count of those events given one or more. The first stage analyzes the inflation factors associated with *any* participation. The medical leave and administrative position variables were statistically significant predictors of zero FIO activity during the study time period, controlling for other factors. BPD officers who were not able to perform their duties or were assigned to administrative positions generally do not complete FIO reports.

289. *See infra* Table 8.

290. *See infra* Table 8.

Table 8. Negative Binomial Regression of the Number of FIO Reports by Officer Race and YVSF Status (IRR, SE, *p*)

	<i>Model 1</i>	<i>Model 2</i>
Age	.916 (.006) **	.922 (.006) **
Female	.307 (.059) **	.383 (.074) **
White Officer	1.752 (.335) **	----
Black Officer	1.171 (.243)	----
Hispanic Officer	1.613 (.338) *	----
White YVSF	----	9.022 (2.136) **
White Other	----	1.488 (.287) *
Black YVSF	----	8.358 (2.081) **
Black Other	----	.826 (.170)
Hispanic YVSF	----	10.788 (3.706) **
Hispanic Other	----	1.112 (.265)
Constant	191.969 (37.743) **	175.144 (34.663) **
Observations	1,750	1,750
Log Pseudo-likelihood	-9,245.30	-9,116.84
Wald Chi-Square	312.99	652.49
Wald Chi-Square <i>p</i>	0.000	0.000
Notes: Models estimated with robust standard errors, not clustered due to mobility of officers. Officers included in this analysis made at least one FIO report between 2007 and 2010. Asian is the contrast category for the FIO officer race tests.		
Significance: + $p \leq .10$, * $p \leq .05$, ** $p \leq .01$		

Model 1 in Table 8 shows, for this narrower sample of officers, that White and Hispanic officers had substantially more FIO encounters than Black officers.²⁹¹ Without controlling for assignment, the effect size for White officers is more than three times the size for Black officers; the effect size for Hispanic officers is more than three times the size for White officers.²⁹² Model 2 shows that this effect is an artifact of YVSF assignment.²⁹³ Within officer race, YVSF officers have far more frequent FIO activity than their non-YVSF counterparts.²⁹⁴ The differences again are very large. White YVSF officers have about 6.5 times more FIO encounters per month than

291. *See supra* Table 8.

292. *See supra* Table 8.

293. *See supra* Table 8.

294. *See supra* Table 8.

White officers in other units.²⁹⁵ The differences for Black and Hispanic officers in the YVSF units are even greater.²⁹⁶

Here again, we see the importance of the YVSF unit in explaining racial disparities in FIO encounters between citizens and police. This is not to say that there is no evidence of racially disparate treatment by officers in other commands; the data show that in fact, regardless of command, White officers and Hispanic officers are more active in FIO work.²⁹⁷ Rather, Table 8 shows that within this focus of police efforts, the race disparities within officer racial categories are quite large, and officers from all racial and ethnic groups are more active once assigned to this command. The results suggest an institutional dimension to explain officer FIO activity that is separate from an individual officer's taste or preference for discrimination.

5. *Frisks and Searches by Officer Race and Assignment*

Table 9 shows differences in frisk / search probability by officer race and assignment.²⁹⁸ Black officers were 15.0% less likely to frisk / search subjects during FIO encounters when compared to White officers, controlling for age, sex, rank, detective status, and assignment.²⁹⁹ Asian officers were also less likely to frisk / search FIO subjects.³⁰⁰ Relative to White officers, Asian officers were 32.6% less likely to frisk / search subjects during FIO encounters controlling for officer demographic, rank, and assignment covariates.³⁰¹ Hispanic officers were only 4.4% less likely to frisk / search subjects during FIO encounters holding the other variables constant; that result was not statistically significant.³⁰² More experienced officers and female officers were significantly less likely to frisk / search subjects during FIO encounters relative to their younger and male counterparts, respectively, controlling for assignment, rank, and other factors.³⁰³

295. *See supra* Table 8.

296. *See supra* Table 8.

297. *See supra* Table 8.

298. *See infra* Table 9.

299. *See infra* Table 9.

300. *See infra* Table 9.

301. *See infra* Table 9.

302. *See infra* Table 9.

303. *See infra* Table 9.

Table 9. Hierarchical Logistic Regression Estimating Impact of Officer Race on Probability of a Frisk or Search (OR, SE, <i>p</i>)			
<i>Characteristic</i>	OR	SE	<i>p</i>
Years on Job	0.973	(.007)	**
Female	0.618	(.069)	**
<i>Officer Race</i>			
Black	0.850	(.066)	**
Hispanic	0.956	(.156)	
Asian	0.674	(.121)	**
<i>Officer Rank</i>			
Detective	1.495	(.187)	
Sergeant or Lt.	0.847	(.151)	
Captain or Command	0.5	(.133)	*
<i>Officer Unit</i>			
YVSF	1.243	(2.655)	**
Constant	315.322	(49.720)	**
Observations	200,103		
Log Likelihood	-123,410.23		
Wald Chi-square	1,618.47		
<i>p</i> (Chi-square)	0.000		
Notes: Robust standard errors clustered by police district. Random effects (not shown) included census tract population (logged), total crime in tract (logged, lagged), disadvantage index, and Moran's I. Fixed effects for year, season, and police district. Significance: ⁺ <i>p</i> < .10, * <i>p</i> < .05, ** <i>p</i> < .01			

Two assignments show extremely elevated rates of frisk / search activity. Detectives were 49.5% more likely to frisk / search subjects during FIO encounters relative to non-detectives, controlling for assignment, rank, and other factors.³⁰⁴ Given their responsibility for investigating unsolved crimes, detectives were presumably more likely to frisk / search FIO subjects for evidence of criminal activity during the course of an investigation. YVSF officers were 24.3% more likely to frisk / search subjects during FIO encounters relative to non-YVSF officers, controlling for assignment, rank, detective status,

304. See *supra* Table 9.

and other factors.³⁰⁵ YVSF officers focus FIO encounters on gang members who pose a higher risk of carrying weapons relative to other FIO subjects, which explains in part their preferences for search relative to other BPD officers.³⁰⁶ Compared to line level patrol officers, Sergeants, Lieutenants Captains, Deputy Superintendents, and Superintendents were significantly less likely to frisk / search subjects during FIO encounters holding other officer characteristics constant.³⁰⁷

Despite the frequent FIO activity by YVSF officers, these results suggest that they exercise caution in proceeding from an encounter to a frisk or search. YVSF officers were far more active in FIO activity, by orders of magnitude, than their non-YVSF counterparts, yet only a fraction of their encounters proceeded to a frisk or search.³⁰⁸

The disparity between FIO encounters by this group and frisks or searches could suggest problems in their formation of the requisite suspicion necessary to conduct a frisk or search contingent on a stop. The high rate of non-frisk encounters suggests a reduced level of reasonable suspicion in many encounters that falls below constitutional thresholds permitting a frisk—primarily officer safety or suspicion of weapon possession—or a search. Searches require probable cause, a stricter standard.³⁰⁹ Another interpretation of this gap could simply be that the purpose of YVSF encounters is simply to establish contact, to signal to young males under suspicion that the police are present and watching, and to gather intelligence. This may be a reasoned activity in terms of policy, but it falls short of being *reasonable* under constitutional requirements for even a momentary deprivation of liberty and detention. ‘Getting it wrong’ at a high rate suggests problems in the bases of suspicion animating a stop, a finding with implications for constitutional regulation of FIO activity.³¹⁰

6. Officer-Suspect Racial Asymmetries

The higher incidence of FIO encounters for non-White suspects and also encounters initiated by White officers suggest the possibility

305. See *supra* Table 9.

306. See Manning, *supra* note 268.

307. See *supra* Table 9.

308. See *supra* Table 9.

309. See *Commonwealth v. Torres*, 745 N.E.2d 945, 948 (Mass. 2001) (stating that Massachusetts follows constitutional standards in *Terry v. Ohio*).

310. See *Floyd v City of New York*, 959 F. Supp. 2d 540, 579 (S.D.N.Y. 2013) (finding that the high rate of unproductive stops was a sign of inaccuracy in the formation of reasonable suspicion that is a prerequisite to an investigative stop).

of discrimination. But these results leave open the question of statistical versus preference-based discrimination. Most studies testing for discrimination, particularly those analyzing vehicle stops, have relied on “hit rates” or the probability of guilt to distinguish between these forms of discrimination. Generally, we assume an absence of preference-based discrimination if racial differences in police encounters are independent of the race of the police officer.³¹¹ But if there are differences in, for example, the stops of Black suspects by White officers compared to Black officers, we might conclude preferences for discrimination. But that evidence alone is only a partial explanation. Those preferences might be explained by the greater ease with which officers may be able to approach and conduct searches of persons of their own race or ethnicity. Alternately, if officers are not randomly assigned to neighborhoods, then Black officers in White neighborhoods where crime rates may be lower will conduct fewer stops of Whites. Since crime rates are higher in predominantly Black neighborhoods in Boston and other cities, the opposite condition would be observed: White officers would have more encounters with Black suspects. Testing for discrimination using these metrics therefore requires not only knowledge of officer and suspect race, but also controls for the crime rates of the different areas where they patrol and encounter suspects.³¹²

Table 10a shows the results of analyses that disaggregate patterns of FIO encounters by both officer race and suspect race four racial groups.³¹³ We estimated models of the count of FIO encounters using negative binomial regressions, following the functional form used in the previous models of FIO activity. Controls included age and gender of the suspect and age, gender, rank and assignment for officers. Separate models were conducted for each officer race group. Fixed effects for police districts controlled for differential exposure of officers to crime and to different local racial concentrations. The first three columns compare FIO reports of each suspect racial group by

311. See generally Shamena Anwar & Hanming Fang, *An Alternative Test of Racial Profiling in Motor Vehicle Searches: Theory and Evidence*, 96 AM. ECON. REV. 127 (2006); see also Antonovics & Knight, *supra* note 146, at 2; Bjerk, *supra* note 146, at 525.

312. This approach also discounts the problem of the “suspicious outsider,” or the person who crosses neighborhoods of different racial composition. See Antonovics & Knight, *supra* note 146, at 25. This problem may be more salient in studies of vehicle stops where crossing of neighborhood boundaries is more common and feasible. Here, our analysis examines pedestrian stops almost exclusively.

313. See *infra* Table 10a.

officers of each race to FIO reports done by White officers.³¹⁴ The fourth column compares FIO's by White officers to FIO reports of Black Officers.³¹⁵ The cells in Table 10a show the incidence rate ratio for each comparison.³¹⁶

Table 10a. Negative Binomial Regression Analyses of the Joint Distribution of Officer Race and Subject Race on FIO Counts (IRR, SE)				
<i>Subject Race</i>	<i>Officer Race</i>			
	<i>Black</i>	<i>Hispanic</i>	<i>Asian</i>	<i>White</i>
Black	.645** (.071)	.865 (.139)	.504** (.112)	1.548* (.169)
Hispanic	.581** (.063)	.128 (.170)	.664 (.171)	1.722** (.188)
Asian / Other	.616** (.089)	1.219 (.334)	1.113 (.281)	1.623** (.235)
White	.426** (.041)	.731* (.103)	.702* (.200)	2.345** (.227)
Note: Models estimated with robust standard errors clustered by police district. Estimates control for suspect and officer age and gender. Fixed effects include year, season, police district, and officer rank and assignment. White is the contrast category for officer race variables in the regressions in the first three columns of coefficients. Black is the contrast category for the White officer race dummy variable in the regressions in the fourth column.				
Significance: ⁺ $p < .10$, * $p < .05$, ** $p < .01$				

To test for different discrimination patterns in frisks and searches, we use multilevel logistic regression models as the functional form to estimate the probability of a frisk or search across racial groups. The results in Table 10b show the odds ratio for each comparison.³¹⁷

314. *See infra* Table 10a.

315. *See infra* Table 10a.

316. *See infra* Table 10a.

317. *See infra* Table 10b.

Table 10b. Hierarchical Logistic Regression Analyses of the Joint Distribution of Officer Race and Subject Race on the Likelihood of a Frisk / Search (OR, SE)				
<i>Subject Race</i>	<i>Officer Race</i>			
	<i>Black</i>	<i>Hispanic</i>	<i>Asian</i>	<i>White</i>
Black	.813** (.014)	.922** (.020)	.649** (.038)	1.229** (.021)
Hispanic	.991 (.041)	.968 (.040)	.605** (.068)	1.008 (.041)
Asian / Other	.949 (.060)	1.031 (.071)	.724* (.112)	1.052 (.066)
White	.874** (.032)	.926* (.035)	.811** (.057)	1.143** (.042)
Note: Models estimated with robust standard errors clustered by police district. Estimates control for suspect and officer age and gender. Fixed effects include year, season, police district, and officer rank and assignment. White is the contrast category for officer race variables in the regressions in the first three columns of coefficients. Black is the contrast category for the White officer race dummy variable in the regressions in the fourth column. Significance: ⁺ $p < .10$, * $p < .05$, ** $p < .01$				

Table 10a shows higher FIO activity for White officers for suspects of all races, including White suspects, compared to Black officers.³¹⁸ White officers have significantly more encounters with White suspects than they have with suspects of other races.³¹⁹ On the surface, this suggests greater FIO activity compared to Black officers across all suspect race groups, but not preferences for stops of one racial group over others. Column 1 shows that Black officers, compared to White officers, are significantly less active across all suspect race groups, again suggesting discrimination other than preference-based.³²⁰

However, comparing within-suspect race results across rows, suggests preferences for discrimination by White officers. FIO activity Column 4 in Table 10a shows that White officers have about

318. *See supra* Table 10a.

319. *See supra* Table 10a.

320. *See supra* Table 10a.

55% more FIO encounters per month with Black suspects compared to Black officers.³²¹ Black officers have 35% fewer stops per month of Black suspects compared to White officers.³²² This between-officer within-suspect comparison suggest preferences by White officers compared to Black officers in FIO activity for Black suspects. Similar differences are evident between Black and White officers in stops of Hispanic suspects, Asian suspects, and White suspects.³²³

The pattern for frisks and searches in Table 10b is similar. White officers are more likely to frisk or search both Black and White suspects compared to cross-racial frisks or searches by Black officers.³²⁴ Black officers again show lower rates of frisks and searches compared to White officers, and are equally likely to frisk or search both White and Black suspects.³²⁵ White officers are 23% more likely to frisk or search a Black suspect, but Black officers are 19% less likely to search Black suspects compared to White officers.³²⁶ Hispanic officers are less likely compared to White officers to frisk Black and White suspects, while White officers are more likely than Hispanic officers to frisk or search both Black and White suspects.³²⁷

One way to understand Table 10a is that while White officers may not discriminate between suspects of different races, they do have stronger preferences for stops between races than Black officers. This is evident for suspects of all races. This presents a more complex picture of the preference-statistical discrimination distinction than previous studies have reported. White officers are more active than are Black or Hispanic officers in FIO activity overall, but they also prefer within each separate race to conduct FIOs relative to Black officers. There may not be preferences by race, but there does appear to be stronger preferences for FIO activity overall. Put another way, White officers are biased toward everyone compared to Black, Hispanic or Asian officers.

Given the higher rates of FIO encounters by YVSF officers, we tested to see if the results in Tables 10a and 10b would be robust to the exclusion of those officers. The results led to the same conclusions, with only minor changes in the regression coefficients

321. *See supra* Table 10a.

322. *See supra* Table 10a.

323. *See supra* Table 10a.

324. *See supra* Table 10b.

325. *See supra* Table 10b.

326. *See supra* Table 10b.

327. *See supra* Table 10b.

and standard errors once the YVSF officer were excluded. We observed the same mixed pattern of statistical and preference-based discrimination that analyses with the full sample produced. In other words, any preferences that we observed were not limited to that active command.

CONCLUSION

Two features of Boston's practices of investigative stops distinguish it from the "new policing" regimes in other large cities. First, Boston focuses a significant portion of its field investigation activity on suspected and actual gang members. Boston police have pursued this targeted strategy within its FIO activity for quite some time.³²⁸ This is consistent with the elevated rates of crime, especially youth crime, in the neighborhoods with the highest concentration of gang members.³²⁹ In contrast, New York City's investigative stop program only recently reoriented from widespread investigative stops to a more spatially concentrated effort focused on gang activity in public housing sites.³³⁰ Second, the Boston *Terry* stop design includes both contact encounters and non-contact observations of suspects. Intelligence and surveillance may not be unusual in other cities, but rarely is there formal recording of observations that do not convert into contact encounters. Boston is distinctive in recording those observations in the same database as its contact encounters.

The records of these encounters provided a basis to assess the claims of discrimination that have infected the contemporary practice of *Terry* stops or investigative stops as practiced in the "new policing." We conducted analyses to assess the allocation of officers and FIO activity by neighborhood and suspect race, using metrics and methods that were cited in recent litigation on other investigative stop

328. See generally DAVID KENNEDY ET AL., U.S. DEP'T OF JUS., REDUCING GUN VIOLENCE: THE BOSTON GUN PROJECT'S OPERATION CEASEFIRE SERIES RESEARCH REPORT 2 (2001).

329. See *Detering Gang Gun Violence*, *supra* note 269, at 2; *The Corner and the Crew*, *supra* note 269, at 422; *Losing Faith?*, *supra* note 269, at 146–47.

330. Richard Aborn, *Crime Commission Statement on NYPD's Operation Crew Cut*, CITIZENS CRIME COMM'N N.Y.C. (Oct. 2, 2012), www.nycrimcommission.org/pdfs/ccc-10-02-12.pdf [<https://perma.cc/56QZ-TVFD>] ("Operation Crew Cut is a smart and proactive approach to curtail youth violence which accounts for 30% of shootings in New York City. By utilizing the latest technology, doubling the size of the gang violence unit and coordinating closely with District Attorneys, the New York City Police Department is making effective use of targeted resources to not only combat crime, but create an overall deterrent effect. This is the kind of smart solution which keeps our crime rate low, and continues to set New York apart as one of the safest large cities in the world.").

programs.³³¹ Both crime and race contribute to variations over time and place in FIO activity. The regressions are estimated so that effects of each are mutually adjusted. The results are robust to the exclusion of the specialized and very active YVSF gang unit, suggesting a generalized pattern of preferences for encounters with Black and Hispanic suspects. We expect a rational allocation of police activity to match variation in times and places with local crime rates, and we observe that to be the case. But we also observe a marginal effect of racial composition in census tracts, suggesting statistical discrimination in those areas.

The evidence suggests a complex answer to the question of whether that discrimination is evidence of bias. Using a racial mismatch model, we find that White officers were consistently more active than Black or Hispanic officers in conducting FIO reports, regardless of suspect race. But within suspect race, the preference of White officers to FIO Black suspects is far greater than Black officers' preference to FIO Black or Hispanic suspects. And Black officers are less likely to FIO a White suspect than is a White officer. These patterns are robust to the exclusion of YVSF officers, which suggests that the results also are robust to the exclusion of gang members.

Is this evidence of bias, or preference-based discrimination? The data are not well suited to answer this question. We defined statistical discrimination as a rational decision to focus efforts on one group or to exclude another group from engagements of any sort. Following Gary Becker's notion of discrimination, officers who stopped Black or Hispanic suspects more often perceived a net benefit in the form of increased attention to crime detection.³³² As a matter of efficiency, attention to populations with a lower probability of return would sacrifice the returns in crime control from allocating attention to the presumed higher rate group. Becker also identifies discrimination based on tastes or values, where the decision maker discounts known facts.³³³ In the case of FIO activity, decision makers may inflate crime propensity beyond its true value, leading to a subjectively rational but still preference-based form of discrimination. In our study, the marginal rate of FIOs by census tract based on racial

331. *See* *Floyd v. City of New York*, 959 F. Supp. 2d 540, 579 (S.D.N.Y. 2013). *See generally* IAN AYRES & JONATHAN BOROWSKY, ACLU OF SOUTHERN CALIFORNIA, A STUDY OF RACIALLY DISPARATE OUTCOMES IN THE LOS ANGELES POLICE DEPARTMENT (OCT. 2008); Plaintiffs' Fifth Report to Court and Monitor on Stop and Frisk Practices, *Mahiri Bailey et al. v City of Philadelphia*, C.A., No. 10-5952, (E.D.Pa. 2015).

332. *See* GARY S. BECKER, *THE ECONOMICS OF DISCRIMINATION* 197 (1971).

333. *See id.* at 16.

composition after controlling for local crime rates may reflect that type of process. It suggests that subjective evaluations of the returns from a FIO encounter may be inflated based on race-based distortions of information.

Kenneth Arrow described this process as “positive valuation” of one group with higher expected return, even if that valuation is inflated.³³⁴ Arrow describes sources of “cheap information” that might help a decision maker to identify a discrimination target a low cost: skin color, poor neighborhoods, or other substitutes for crime.³³⁵ These sources of cheap information may also prime decision makers—police officers, in this case—to increase their valuation of the suspect’s behavior. Since there is no cost for a wrong decision, there are only weak incentives to correct or update that information.

Using the officer-suspect racial mismatch metric, the results suggest preference-based discrimination. But without additional evidence of the outcomes of FIO encounters, or the reasonable suspicion bases animating these events, we cannot fully explain the motivations for FIO encounters. These encounters may be efficient, or they may simply be a form of routine administrative searches based on actuarial suspicion in the absence of individualized or particularized suspicion³³⁶ or a clear expectation of arrests or seizures of weapons.³³⁷ These encounters may simply reflect an institutional bias or norm based on a closed system of information that reinforces command staff and individual officers’ prior beliefs about whom to observe or engage, setting aside questions of fairness or efficiency. FIOs here may be based on location, peer network, or other actuarial markers that substitute for individual markers of suspicion, raising Fourth Amendment concerns. Explaining how those factors translate into

334. See Kenneth J. Arrow, *The Theory of Discrimination*, in DISCRIMINATION IN LABOR MARKETS 4 (Orley Achenfelder & Albert Reiss eds., 1973).

335. See *id.*

336. See Eve Bresinke Primus, *Disentangling Administrative Searches*, 111 COLUM. L. REV. 254, 287 (2011) (defining administrative search exceptions to Fourth Amendment regulation to justify airport searches, subway backpack searches, employer drug testing, and vehicle checkpoints).

337. See Brooks Holland, *The Road ‘Round Edmond: Steering Through Primary Purposes and Crime Control Agendas*, 111 PA. ST. L. REV. 293, 295 (2006) (citing the U.S. Supreme Court opinion in *Edmond v City of Indianapolis*, 531 U.S. 37, 44 (2000), stating that “[w]e cannot sanction stops justified only by the generalized and ever-present possibility that interrogation and inspection may reveal that any given motorist has committed some crime”).

perceptions, decisions, attributions or behaviors requires different research designs.³³⁸

Still, evidence of officer race disparities suggests that there is more than just statistical discrimination or institutional preference at work here. That these stops disproportionately target minority suspects in non-White neighborhoods beyond what local crime rates predict, raises Equal Protection concerns that seem to be collateral consequences of the “new policing.”

338. Several studies have used research designs that vary race and use a variety of cues to assess how race consciously or subconsciously affects decision making by legal actors. These studies differ from the officer-suspect mismatch paradigm in that they examine specific cues that influence officers' perceptions and permit bias to infect decisions from shooting at suspects to the construction of pre-sentence probation reports for trial courts. Many are laboratory experiments, which strengthens their internal validity but to some extent at the cost of external validity considerations of context and multiple causation. Others exploit natural variation in legal settings to discern the influence of race on decision making, increasing their external validity but at some unknown cost to internal validity and measurement equivalence on race. In most but not all cases, these studies show evidence of bias toward African-American suspects or defendants. *See generally, e.g.*, Modupe Akinola & Wendy Berry Mendes, *Stress-Induced Cortisol Facilitates Threat-Related Decision Making among Police Officers*, 126 BEHAV. NEUROSCIENCE 167 (2012); George S. Bridges & Sara Steen, *Racial Disparities in Official Assessments of Juvenile Offenders: Attributional Stereotypes as Mediating Mechanisms*, 63 AM. SOC. REV. 554 (1998); Joshua Correll et al., *Stereotypic Vision: How Stereotypes Disambiguate Visual Stimuli*, 108 J. PERSONALITY & SOC. PSYCHOL. 219 (2015); Joshua Correll et al., *The Police Officer's Dilemma: A Decade of Research on Racial Bias in the Decision to Shoot*, 8 SOC. & PERSONALITY PSYCHOL. COMPASS 201 (2014); Jennifer L. Eberhardt et al., *Looking Deathworthy: Perceived Stereotypicality of Black Defendants Predicts Capital-Sentencing Outcomes*, 17 PSYCHOL. SCI. 383 (2006); Sandra Graham & Brian S. Lowery, *Priming Unconscious Racial Stereotypes about Adolescent Offenders*, 28 L. & HUM. BEHAV. 483 (2004); Alpert, *Police Suspicion and Discretionary Decision Making during Citizen Stops*, *supra* note 98.